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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers in the present Volume are, Sir H. C. Englefield, Bart. M. P. F. R. S.; H. B. K.; A Correspondent; W. N.; Mr. Sylvester; J. Dalton; J. Kidd, M. D.; J. Bostock, M. D.; Mr. J. Arnold; M. M.; Juvenis; T. B.; James Horsburgh, Esq.; G. S. Gibbes, M. D.; R. B.; Dr. Buchan; Dr. Wilkinson; H. Hamill, jun. Esq.; H. K.; Count Rumford, F. R. S.; Mr. Thomas Reid; Mr. W. Hardy; Dr. Thompson.

Of Foreign Works, Professor Proust; M. Bouillon Lagrange; M. Darcet Cointeraux; Thenard; J. F. Westring; Lavoisier; Laplace.

And of English Memoirs abridged or extracted, Sir James Hall, Bart. F. R. S. &c.; Mr. John Arnold; T. Young, M. D. For. Sec. R. S.; T. A. Knight, Esq.; Thos. Holden; Mr. T. Vanherman; Mrs. Jane Richardson; H. Davy, Esq. F. R. S.; W. Brande, Esq.; Mr. Earnshaw.

Of the Engravings the Subjects are, 1. A new Lamp, by Count Rumford: 2. The Escapement of Arnold's Chronometer: 3. Arnold's Expansion Balance for Time-pieces: 4. The Water Ram of Mongolfier: 5. Holden's Machine for Shoe-making; 6. A Galvanic Apparatus, by Mr. Sylvester: 7. Designs for Furnaces, and Apparatus for ascertaining the Force of Compression, by Sir James Hall, Bart. F. R. S. Ed.: 8. Sections and Drawings to illustrate the Huttonian Theory as established on Sir James Hall's Theory: 9. Tools and Implements used in building Houses of rammed Earth: 10. Drawing of a Building constructed by that Art: 11. The Thermometrical Balance of Peter le Roy: 12. Expansion Balance, by the same Artist: 13. Le Roy's modern detached Escapement: 14. The same improved; 15. The same as altered by Berthoud: 16. The same by Arnold: 17. The same by Earnshaw: 18. Ancient free Escapement, by Thiout: 19. New Apparatus by Count Rumford for Experiments on Heat: 20. Mr. Harding's Improvement in Time-pieces: 21. The revolving Balance of Robert Hook: 22. Diagram, by Mr. Brewster, concerning Achromatic Eye Pieces: 23. Another by Robert Hooke concerning Pendulums.

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AUGUST 1806.

SUPPLEMENT.

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MAY, 1806.

ARTICLE I.

Account of a simple and cheap portable Barometer, with Instructions to enable a single Observer to determine Heights by that Instrument with considerable Facility and Precision. In a Letter from SIR H. C. ENGLEFIELD, *Bart. M. P. F. R. S, &c. &c.*

To Mr. NICHOLSON.

SIR,

THE mensuration of heights by the barometer has been, by the labours of Mr. De Luc, Sir George Shuckburg, General Roy, and several other scientific men, brought to such perfection, and affords so much an easier mode of ascertaining the elevations of the different parts of the surface of the earth to a considerable precision, than any other known process, that it might have been supposed that, in the course of thirty years which have elapsed since this branch of science has been perfected, a very great number of observations would have been made, and the heights of almost the whole surface of our own country ascertained by the numerous travellers who continually

Though the mensuration of heights by the barometer has been greatly perfected, yet the observations are few.

ally traverse it. The contrary is however the case, and the small number of observations of this kind may be attributed to several causes. The instruments are of considerable expence, and from their complicated construction easily liable to be out of order in the course of a long journey. The observations themselves, though each not taking up any very long time, yet when multiplied on every hill and valley, as they ought to be, for the purpose of obtaining a just idea of the country surveyed, in the aggregate consume much of the traveller's time, and the constant unpacking and repacking the instrument becomes a greater labour than our natural indolence easily submits to.

Causes. The instruments are costly, liable to be deranged, and the observations demand time and care.

It has moreover been generally supposed, that two instruments and two observers making simultaneous observations at the upper and lower stations of the height to be measured, are indispensably necessary. This of course would put it out of the power of a solitary traveller to make any observations at all. Whether from these or other causes, the fact is, that whoever reads the numerous tours, surveys, and reports of different parts of our island published within these last twenty years, and many of them professedly with a view to science, either of agriculture, mineralogy, or geology, will be perpetually disappointed by meeting with mere guesses at the elevations of the tracts of country described, though a knowledge of those elevations is almost indispensable to the geologist, mineralogist, and military surveyor,—highly useful to the scientific agriculturist, and very interesting to every one who from mere motives of enlarged and enlightened curiosity reads books of travels, or employs his own leisure in traversing the countries described by other voyagers.

Two observers with separate instruments have generally been supposed necessary.

Hence it is that the elevations and depressions of the face of the earth, tho' of high utility and interest, are seldom noted in travels.

I cannot therefore but hope, that by simplifying the barometer, and thereby rendering the instrument much less expensive and its use at the same time more easy, and shewing that very considerable accuracy may be attained by a single observer, this most useful branch of science may be cultivated to so great an extent, that in the course of a few years we may have almost as perfect an idea of the relative heights of the different parts of England as we now have of their horizontal distance.

The impediments to observation proposed to be removed by simplifying the instrument and shewing that one observer may obtain valuable results.

A barometer, nearly similar to that which I am now about to describe,

Portable barometer of Dr. Hugh Hamilton.

describe, was constructed several years since by Dr. Hugh Hamilton, and is by him described at large in the fifth volume of the Transactions of the Irish Academy. I saw the instrument in his hands, nearly sixteen years ago, and was much pleased with its performance. I do not know, however, that any more were then made. I have lately constructed the barometer, whose description I shall now give, which is still more simple than Dr. Hamilton's, and much cheaper; and which, in many trials I have made of it, appears to unite solidity, lightness, and ease of observation to as great a degree as can be wished.

—Simplified by the author. It is light, firm, and easy in the use.

The barometer tube is about 33½ inches in length; its bore is one-tenth of an inch in diameter, and the external diameter is three-tenths of an inch. This sized bore is fully sufficient to allow the free motion of the mercury. The cistern is of box, turned truly cylindrical, and is an inch in its internal diameter, and an inch in depth. A short stem projects from its top, (the instrument being in the position for making an observation,) for the purpose of giving a firmer hold to the tube. This stem is perforated with an hole sufficiently large to admit the tube, which is glued to it in the usual mode. The tube projects into the cistern exactly to half its depth. The bottom of the cistern is closed by a strong lid of box, which screws on the cistern, and pressing against a leather glued to the inside of the lid renders the whole perfectly impervious to the mercury in every position. The tube being filled and boiled in the common way, and the instrument held inverted in a perpendicular position, mercury is poured into the cistern till it is filled within two-tenths of an inch of the top. The lid is then firmly screwed on, and secured from being opened by idle curiosity by a small screw passing through its side. The essential part of the instrument is now finished. The end of the tube in the cistern can never be uncovered by the mercury in any possible position, and of course no air can ever enter into it: and as the areas of the cistern and tube are as the squares of the diameters, the diameter of the bore of the tube being ,1, its external diameter ,3, and the diameter of the cistern 1,0, the area of the cistern is 100 — 9 = 91; and there being two-tenths of an inch left empty in the cistern, the mercury must fall 182-tenths, or 18 inches and 2-tenths, before the cistern is quite full: a space adequate to the measure of greater heights than any

Description. The barometer tube is glued into a box cylinder, the bore of which exceeds that of the tube as 100 to 9 in area. The tube reaches to the middle of the length of the cylinder, the other end of which cylinder is closed with a screw-cap, and leather. When the tube is filled, mercury is also poured into the cylinder, so as to occupy three-fourths its length, and therefore covers the end of the tube in all positions.

When this barometer is set upright the atmosphere acts thro' the pores of the wood.

any known mountain on the earth, much more so to any height in this country. It will not easily be believed by those who have not seen it, that the air will act on a cistern thus completely closed, and of which the wood in its thinnest part is above a quarter of an inch in thickness: but the fact is, that even when the pores of the box-wood are closed by thick varnish, except in that part which touches the mahogany tube, in order to prevent the wood from being affected by damps, the mercury on turning up the barometer takes its level almost instantaneously, certainly in less than half a minute; and that when the instrument is suspended by the side of the best mountain barometer of Ramsden's, constructed with an open cistern, no difference whatever can be perceived in their sensibility to the variations of the atmosphere. It is obvious that the variations of altitude in this instrument of dimensions above stated, will be one ninety-first part less than in a barometer furnished with an apparatus for bringing the surface of the mercury in the cistern to a fixed level: this defect might be remedied by dividing the scale accordingly; but it is much more convenient to divide the scale to real inches, and make the necessary allowance in the result.

The barometer is mounted in a mahogany tube of the size of a walking-stick.

The tube and cistern being thus prepared, are mounted in a mahogany tube of the size of a common walking-stick. The stem of the cistern goes tight into the mahogany tube, and is there secured by two small brass screws, or the stem may be on the outside cut into a male screw, and so be screwed into the mahogany tube. The cistern forms an head or pummel to the staff when the instrument is inverted for the purpose of being carried in the hand or a carriage. The tube is secured in the mahogany case by passing through perforated corks in the usual way.

Means and method of observing the height of the mercury.

For the observation of the height of the mercury, two opposite slits are cut in the mahogany tube, reaching from about 32 to 20 inches, for the longer scales, or from 32 to 25 inches for such as are intended for use in this country. The front slit has its sides bevelled, and is exteriorly about three-fourths of an inch wide. On one side is fixed a brass plate, divided as usual to inches, tenths, and twentieths. On this plate a nonius slides moveable by a small knob, which reads off as in other barometers, to the 500th of an inch. To this nonius a small portion

portion of brass tube is attached, which embraces the barometer tube, and its lower edge is, in observation, made a tangent to the convex surface of the mercury, as in other well-constructed barometers, and the very narrow slit behind gives abundant light for observation.

Attached and detached thermometers; facilities in the construction, &c.

On the bevelled side of the front slit opposite the scale, a thermometer is placed for taking the heat of the instrument, and there is room for the scale of correction placed on Ramsden's attached thermometers as well as Fahrenheit's scale.

A thin brass tube with slits in it turns half round on two pins in the usual manner, and covers the apertures above described in the mahogany tube when the barometer is not in use. The mahogany tube is made rather tapering, and with a ferril at the end opposite the cistern. This ferril unscrews and shews a steel ring, by which the barometer may be suspended when convenient; and as the mahogany tube is made nearly thirty-eight inches long, there is full space above the top of the barometer tube to put in a thermometer, which is taken out by unscrewing the ferril, and is to be used as a detached thermometer in observation. Along the mahogany tube is a scale of three feet, carefully divided to inches, the feet being accurately laid down by small dots on the heads of brass pins sunk into the wood. A scale of this kind is always convenient, and may often be of great use.

Practical remarks.

Having thus described the instrument, a few practical remarks on the manner of using it may not be superfluous.

To determine the temperature of the air.

When I am about to make an observation, about five minutes before I arrive at the place I take out the detached thermometer from its place in the end of the mahogany tube, holding it by the upper end at nearly arm's-length from my body and, if the sun shines, in the shade of my person: it very soon takes the temperature of the air, and is not sensibly affected by the heat of the hand. The heat being observed and written down, the barometer is turned up, the brass tube half turned, and the instrument held between the finger and thumb of the left hand above the slide, so as to let it hang freely in a perpendicular position. Few persons, if any, have sufficient steadiness of hand to prevent little vibrations in the mercury in this position: the hand therefore should be either rested

against

Observation by the barometer.

against any fixed body, or if no such occurs, by kneeling on one knee; the cistern should be let down so as to touch the ground, the left hand holding the barometer in a vertical position, which a little practice will render very easy. The index must then be moved by the knob till its under surface, as before stated, is tangent to the mercury. A few light taps should be given to the tube to ascertain that the mercury is fallen as low as it can. The height being then read off and registered together with that of the attached thermometer, the brass tube is turned back, so as to cover the slits; the instrument gently inverted; and the whole is finished: all this may be done in two minutes.

Deduction of the heights to be made as usual by the logarithmic tables,

or by other methods.

The most convenient mode for deducing the heights from the barometrical observations, is certainly by the common logarithmic tables; and it is unnecessary here to detail the method, which may be found in numerous books. It is, however, necessary for this method, to carry the tables of logarithms, which is sometimes inconvenient. The engraved table, formed by Mr. Ramsden, is on a single narrow sheet, and extremely portable, besides being very easy in its use; but it may be lost or mislaid when wanted. Several ingenious formulæ have been devised, which may either be engraven on the instrument itself, or committed to memory. Of the former, Sir G. Shuckburg has given a very concise one in his second paper on the measurement of heights by the barometer, in the sixty-eighth vol. of the Philosophical Transactions; and Mr. Professor Leslie has invented a very elegant one of the latter sort: but these, though very simple in form, require a considerable number of figures in the operation, and are on that account inconvenient. For the purpose therefore of computing on the spot, and very near to the truth, any observations made on a journey, and that almost without the necessity of writing at all, I have caused the following short table to be engraven on the scale of the barometer. It expresses the value of the difference of a tenth of an inch in the height of the mercury, at the temperature of freezing, in English feet.

Computations may be made on a journey very near the truth, from a table here given, which is engraven on the barometer.

TABLE.

TABLE.

Inch 10th	Feet	Inch 10th	Feet	Inch 10th	Feet
20 . 05	130	23 . 05	113	27 . 15	96
. 20	129	. 25	112	. 45	95
. 35	128	. 45	111	. 75	94
. 50	127	. 65	110	28 . 05	93
. 66	126	. 87	109	. 35	92
. 82	125	24 . 10	108	. 65	91
21 . 00	124	. 32	107	. 95	90
. 18	123	. 55	106	29 . 27	89
. 35	122	. 80	105	. 61	88
. 53	121	25 . 05	104	. 95	87
. 70	120	. 30	103	30 . 30	86
. 87	119	. 55	102	. 65	85
22 . 05	118	. 80	101	31 . 00	84
. 25	117	26 . 05	100	. 37	83
. 45	116	. 30	99	. 75	82
. 65	115	. 57	98	32 . 10	81
. 85	114	. 85	97		

Method of using the table.

The method of using it is as follows: 1st. Add the two observed heights of the barometer, and halve the sum to obtain the mean height. 2d. Subtract the lesser height from the greater, the remainder is, of course, the difference in tenths, &c. of an inch. 3d. Enter the table with the mean height, and take out the feet answering to it, making a proportion if the mean height does not exactly answer to a foot. (This proportion may be made by head.) Multiply the number thus obtained by the tenths, &c. of an inch of difference of height. The result will be nearly the number of feet answering to the difference of height between the two barometers at the temperature of freezing. When the lower barometer stands between 29 and 30 inches, and the elevation does not exceed 1500 feet, this rule will give the height within one foot of the result from the logarithmic method. When the elevation is about 3000 feet, the error will be nearly three feet, and at heights greater than 3000 feet the error increases in an higher ratio. It is always in defect. In this country, however, such elevations do not exist, and in those parts where a knowledge of the comparative heights of the different hills is the most generally useful, they seldom exceed 1000 feet. At all events such observations

Its degree of accuracy.

servations as relate to great elevations may be always recomputed by more rigorous methods at leisure.

Correction for the difference of temperature above the freezing point.

The correction of the heights thus obtained, for the temperature of the air above freezing, is, by Sir G. Shuckburg, supposed to be as the height of the thermometer, and to be 2,44 thousandths of the approximate height for each degree of Fahrenheit; additive when the temperature is above freezing, and subtractive when below freezing. General Roy's observations and experiments lead to a supposition that the correction is not exactly as the height of the thermometer, and that at about the temperature of 50° it amounts to 2.5 thousandths, and is less both much above and much below that temperature. For the purpose of immediate computation, I take the correction at 2,5; which, though certainly too great, will in general be productive of very small error, and affords a rule which is easily remembered, and quickly applied. It is this. For every four degrees that the mean temperature of the two detached thermometers exceeds 32°, add one hundredth of the approximate height, as before obtained, to it; for every 40° one-tenth; and so for any greater or lesser number of degrees. I have not hitherto mentioned the correction which, in fact, ought to be the first in order, viz. that, for the difference of temperature of the two barometers themselves: but this correction is, in general, so small as to be safely neglected, and is besides easily to be made from Mr. Ramsden's numbers, which are engraven on the scale of the attached thermometer. It may not be improper to give an example or two of the method already detailed.

Very simple rule for this purpose.

Examples to illustrate the computations, and shew their accuracy.

Observation at bottom,	29.400	Therm. in air,	45
Observation at top,	25.200	Therm. in air,	41
2	54.600	2	86
Mean, —	27.300	Mean heat,	43
		Standard,	32
		Difference,	11

Difference,

Difference,	42 tenths.	
Value of a tenth by table,	95.5 feet.	
	1910	
	3820	
Approximate height,	4011.0 feet.	
Do. by Sir G. Shuckburg,	4016.0	
Error, —	5 feet.	

CORRECTION FOR TEMPERATURE.

For 8° = 2 hundredths,	80 feet.
For 3° = 3 four hundredths,	30
Correction, +	110
Do. by Sir G. Shuckburg,	107.4
Error, +	2.6

Approximate Height	by me.	Sir G. Shuckburg.
	4011	4016
Correction for Temper.	110	107.4
Result, — —	4121	4123.4

EXAMPLE II.

Observation at bottom,	30.017	Therm. in air,	60°
Observation at top,	29.534	Therm. in air,	57
2	59.551	2	117
Mean,	29.775	Mean,	28.5
Difference,	4.83	Standard,	32.
		Difference,	26.5

Value of a tenth by table,	87.5
	350.0
	70.00
	2.625
Approximate Height,	422.625
Do. by Sir G. Shuckburg,	422.9
Error, —	00.3

CORRECTION FOR TEMPERATURE.

For 24°	=	6 hundredths,	25 . 3
2	=	2 four hundredth,	2 . 0
$\frac{1}{2}$	=	1 eight hundredth,	0 . 5
Correction,		+	27 . 8
Do. by Sir G. Shuckburg,			27 . 2
Error,		+	0 . 6

	By me.		By Sir G. Shuckburg.
Approximate height,	422 . 6		422 . 9
Correction for Temper.	27 . 8	—	27 . 2
	459 . 4		450 . 1

These two examples shew how near the truth the method here recommended will come, even in considerable heights.

Estimate of the probable error which may arise from a difference in the proportion of the bore of the tube to that of the cistern; it is probably less than one thousandth part.

It has been already observed, that in observations made with the barometer I have described, a small correction is necessary, on account of the rise in the mercury in the cisterns as the barometer falls. Altitudes being in all cases measured by the differences of the heights of the mercury at the two stations, and these differences being evidently always too small in this barometer, the correction is obviously always additive. As, in constructing different barometers, the interior and exterior diameters of the tube will not always be exactly similar, though the cisterns may be turned always alike, this error, and of course the correction for it, should be in each instrument, deduced from a comparison with a barometer of known accuracy, at different heights. It will probably vary in different instruments from a ninetieth to a seventieth. Indeed, if it were always taken at an eightieth, in instruments constructed as above directed, the possible error could only amount to about one foot on a thousand; a quantity of very little importance.

Observations with one single barometer pos-

It now remains to say a few words on the necessity of two baro-

barometers for the mensuration of heights, and the probable error to be incurred by using a single one. There is no doubt, that where very great accuracy is required, two barometers ought to be used; but even with every precaution, altitudes cannot be taken by barometers sufficiently near for the purpose of carrying water either by pipes or canals, and for the purpose of the geologist, military surveyor, or agriculturist, it is of very little importance whether a mountain is 1000 or 1010 feet high, though it is of the highest utility that he should know whether it is 800 or 1000. I have during the course of many years been in the habit of taking observations of altitudes by a single barometer, and have had many opportunities of repeating my observations on the same hills when the barometer has been at different heights, and when falling or rising during the time of observation; and more than once I have observed heights which had been trigonomically taken by the best instruments, and I can safely say that the difference between these observations have seldom amounted to so much as two feet on an hundred. The mode I use is this:—At setting out I take the height of the mercury, and note the time of observation. I likewise note the time of the second observation, and on returning to the first station, observe again and note the time. If the barometer has altered in the interval, a simple proportion corrects either of the three observations, and reduces the height to what would have been observed had the mercury been stationary. It is true, that this method supposes the motion of the mercury to have been uniform during the interval of observation, but except in very variable weather, which does not very often occur, particularly in summer, when the greater number of these observations will naturally be made, this supposition may be safely made. It is also true, that a traveller has often no opportunity of making a second observation at the spot he set out from. Even in this case, a near approximation may often be made by observing, for example, at a stream on each side of the hill to be measured. If also he observes the barometer repeatedly in the morning before he sets out, and sees its tendency, and does the same at every halt during the day, he will have data whereon to found a nearly accurate correction. But if all this should be out of his power, even under the most unfavourable cir-

sess considerable accuracy.

Account of the method practised by the author.

Observations are highly useful, even under unfavourable circumstances.

circumstances, barometical observations will give a much more accurate idea of the outline of a country than any we now possess; and it should be ever remembered, that observations accurately made, and faithfully recorded, are valuable. The repeated observations of different travellers, though separately defective, will in most cases correct each other, and from the whole very accurate conclusions may be drawn.

Fabrication and price of these barometers.

I have entered into a greater detail than would be necessary for a great part of your readers, in the hope of being intelligible to those who are less acquainted with the subject, and who may wish to employ any instrument-maker for the construction of barometers similar to that which I have described. In justice to a very ingenious young artist, permit me to add, that I have employed in making those which I have, Mr. Thomas Jones, of No. 120, Mount-street, Berkeley-spuare, pupil of the late Mr. Ramsden, and who will furnish them at the price of two guineas and a half without the attached thermometer, three guineas with it, and three guineas and a half with the attached and detached thermometer. Such barometers, however, as are graduated down to 20 inches, will, on account of the additional work, cost five shillings more.

I am, Sir,
Your humble Servant,
H. C. Englefield.

Tilney-street,
April 5, 1806.

Difference of speed in the settling of these instruments: It does not affect their accuracy.

P. S. On comparing several barometers made by Mr. Jones, since this description was written, I find that in some of them the mercury does not take its true height on turning up the instrument, quite so quick as in the two which he first constructed for me. This difference is owing to the greater closeness of fibre in some pieces of box-wood than in others; but it does not affect the accuracy of the instrument. It may not be superfluous to say, that the weight of this barometer is one pound and three quarters; the weight of Ramsden's last improved barometer is four pounds and a half; and that of his earliest form, six pounds and three quarters.

I subjoin a few observations made at the top and bottom of Richmond-Hill, by which the accuracy of this barometer may be fairly estimated.

Dec.

		Bason.	Therm.	Results.	
Dec. 22.	Hill top,	28.710			
	Thames side,	28.868		Feet.	
		158		— 146	doubtful.
Jan. 1.	Hill top,	29,540	44		
	Thames side,	29.686			
		146		— 133	
Jan. 2.	Hill top,	29.708	38		
	Thames side,	29.860			
		152		— 134	
Jan. 31.	Hill top,	29.301	36		
	Thames side,	29.453	37		
		152		— 137	
Feb. 23.	Hill top,	29.758	51		
	Thames side,	29.912			
		154		— 139	
Feb. 24.	Hill top,	30.180	53		
	Thames side,	30.334	54		
		154		— 140	

Observations made on Richmond-hill.

II.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Action of Heat. By SIR JAMES HALL, *Bart. F. R. S. Edinburgh.*

(Continued from page 405, Vol. XIII.*)*

Experiments in which Water was employed to increase the Elasticity of the included Air.—Cases of complete Compression.—General Observations.—Some Experiments affording interesting Results; in particular, shewing a mutual Action between Silex and the Carbonate of Lime.

FINDING that such benefit arose from the increase of elasticity given to the included air in the last-mentioned experiments,

Contrivance by which a very small quantity of water was

included, in order to increase the compression by its reaction as steam.

ments, by the diminution of its quantity; it now occured to me, that a suggestion formerly made by Dr. Kennedy, of using water to assist the compressing force, might be followed with advantage: That while sufficient room was allowed for the expansion of the liquid metal, a reacting force of any required amount, might thus be applied to the carbonate. In this view, I adopted the following mode, which, though attended with considerable difficulty in execution, I have often practised with success. The weight of water required to be introduced into the barrel was added to a small piece of chalk or baked clay, previously weighed. The weight of water required to be introduced into the barrel was added to a small piece of chalk or baked clay, previously weighed. The piece was then dropped into a tube of porcelain of about an inch in depth, and covered with pounded chalk, which was firmly rammed upon it. The tube was then placed in the cradle along with the subject of experiment, and the whole was plunged into the fusible metal, previously poured into the barrel, and heated so as merely to render it liquid. The metal being thus suddenly cooled, the tube was encased in a solid mass, before the heat had reached the included moisture. The difficulty was to catch the fusible metal at the proper temperature; for when it was so hot as not to fix in a few seconds, by the contact of the cradle and its contents, the water was heard to bubble through the metal and escape. I overcame this difficulty, however, by first heating the breech of the barrel, (containing a sufficient quantity of fusible metal), almost to redness, and then setting it into a vessel full of water, till the temperature had sunk to the proper pitch, which I knew to be the case when the hissing noise produced in the water by the heated barrel ceased; the cradle, during the last stage of this operation, being held close to the muzzle of the barrel, and ready to be thrust into it.

Successful experiment. Temp. 24°.

On the 2d of May, I made my first experiment in this way, using the same air-tube as in the last experiment, which was equal in capacity to one-thirtieth of a cubic inch. Half a grain of water was introduced in the manner just described. The barrel, after an hour of red-heat, was let down by a rope and pulley, which I took care to use in all experiments, in which there was any appearance of danger. All was sound. The

The metals rushed out smartly, and a flash of flame accompanied the discharge. The upper pyrometer gave 24°, and the lower one 14°. The contents of the inner tube had lost less than 1 *per cent.*, strictly 0.84. The carbonate was in a state of good limestone; but the heat had been too feeble: The lower part of the chalk in the little tube was not agglutinated: The chalk round the fragment of pipe-stalk (used to introduce the water), which had been more heated than the pyrometer, and the small rod, which had moulded itself in the boll of the stalk, were in a state of marble.

On the 4th of May, I made an experiment like the last, but with the addition of 1.05 grains water. After application of heat, the fire was allowed to burn out till the barrel was black. The metal was discharged irregularly. Towards the end, the inflammable air produced, burnt at the muzzle, with a lambent flame, during some time, arising doubtless from hydrogen gas, more or less pure, produced by the decomposition of the water. The upper pyrometer indicated 36°, and the lower one 19°. The chalk which lay in the outer part of the large tube was in a state of marble. The inner tube was united to the outer one, by a star of fused matter, black at the edges, and spreading all round, surrounding one of the fragments of porcelain which had fallen by accident in between the tubes. The inner tube, with the starry matter adhering to it, but without the coated fragment, seemed to have sustained a loss of 12 *per cent.*, on the original carbonate introduced. But, the substance surrounding the fragment being inappreciable, it was impossible to learn what loss had been really sustained. Examining the little tube, I found its edges clean, no boiling over having taken place. The top of the small lump of chalk had sunk much. When the little tube was broken, its contents gave proof of fusion in some parts, and in others, of the nearest approach to it. A strong action of ebullition had taken place all round, at the contact of the tube with the carbonate: in the heart, the substance had a transparent granular texture, with little or no crystallization. The small piece of lump-chalk was united and blended with the rammed powder, so that they could scarcely be distinguished. In the lower part of the carbonate, where the heat must have been weaker, the rod had acted more feebly on the tube, and was detached from

Another with more water. Temp. 36°.

from it: here the substance was firm, and was highly marked in the fracture with crystalline facettes. Wherever the carbonate touched the tube, the two substances exhibited, in their mixture, much greater proofs of fusion than could be found in the pure carbonate. At one place, a stream of this compound had penetrated a rent in the inner tube, which it had filled completely, constituting a real vein, like those of the mineral kingdom: which is still distinctly to be seen in the specimen. It had then spread itself upon the outside of the inner tube, to the extent of half an inch in diameter, and had enveloped the fragment of porcelain already mentioned. When pieces of the compound were thrown into nitric acid, some effervesced, and some not.

Experiment repeated with more water than before.

I repeated this experiment on the same day, with two grains of water. The furnace being previously hot; I continued the fire during one half-hour with the mufflle open, and another with a cover upon it. I then let the barrel down by means of the pulley. The appearance of a large longitudinal rent, made me at first conceive that the experiment was lost, and the barrel destroyed: The barrel was visibly swelled, and in swelling had burst the crust of smooth oxide with which it was surrounded; at the same time, no exudation of metal had happened, and all was sound. The metals were thrown out with more suddenness and violence than in any former experiment, but the rod remained in its place, being secured by a cord. The upper pyrometer gave 27°, the lower 23°. The contents of the inner tube had lost 1.5 *per cent*. The upper end of the little lump of chalk, was rounded and glazed by fusion; and the letter which I have been in the habit of cutting on these small pieces, in order to trace the degree of action upon them, was thus quite obliterated. On the lower end of the same lump, the letter is still visible. Both the lump and the rammed chalk were in a good semitransparent state, shining a little in the fracture, but with no good facettes, and no where appearing to have acted on the tube. The last circumstance is of consequence, since it seems to shew, that this very remarkable action of heat, under compression, was performed without the assistance of the substance of the tube, by which, in many other experiments, a considerable additional fusibility has been communicated to the carbonate.

These

These experiments, and many others made about the same time, with the same success, clearly prove the efficacy of water in assisting the compression; and results opproaching to these in quality, obtained, in some cases, by means of a very small air-tube, shew that the influence of water on this occasion has been merely mechanical.

The water increased the pressure, and its effect was merely mechanical.

During the following summer and autum 1803, I was occupied with a different branch of this subject, which I shall soon have occasion to mention.

In the early part of last year, 1804, I again resumed the sort of experiment lately described, having in view principally to accomplish absolute compression, in complete imitation of the natural process. In this pursuit, I did not confine myself to water, but made use of various other volatile substances, in order to assist compression; namely, carbonate of ammonia, nitrate of ammonia, gunpowder, and paper impregnated with nitre. With these I obtained some good results, but none such as to induce me to prefer any of these compressors to water. Indeed, I am convinced, that water is superior to them all. I found, in several experiments, made with a simple air-tube, without any artificial compressor, in which a very low red-heat had been applied, that the carbonate lost one or one and a half *per cent.* Now, as this must have happened in a temperature scarcely capable of inflaming gunpowder, it is clear, that such loss would not have been prevented by its presence: whereas water, beginning far below redness to assume a gaseous form, will effectually resist any calcination, in low as well as in high heats. And as the quantity of water can very easily be regulated by weight, its employment for this purpose seems liable to no objection.

Other volatile bodies used for compression; but water is the best.

On the 2d of January 1804, I made an experiment with marble and chalk, with the addition of 1.1 grain of water. I aimed at a low heat, and the pyrometer, though a little broken, seemed clearly to indicate 22°. Unluckily, the muzzle of the large tube, which was closed as usual with chalk, was placed uppermost, and exposed to the strongest heat. I found it rounded by fusion, and in a frothy state. The little tube came out very clean, and was so nearly of the same weight as when put in, that its contents had lost but 0.074 *per cent.* of the weight of the original carbonate. The marble was but

Experiment in which under the same pressure chalk was fused by an high heat, and another part converted into limestone at a lower.

feebly agglutinated, but the chalk was in a state of firm limestone, though it must have undergone a heat under 22°, or that of melting silver. This experiment is certainly a most remarkable one, since a heat has been applied, in which the chalk has been changed to a hard limestone, with a loss less than the 1000dth part of its weight, (exactly $\frac{1}{1351}$); while, under the same circumstances of pressure, though probably with more heat, some of the same substance had been brought to fusion. What loss of weight this fused part sustained, cannot be known.

Similar experiments.

On the 4th of January, a similar experiment was made, likewise with 1.1 grain of water. The discharge of the metal was accompanied with a flash of flame. The pyrometer indicated 26°. The little tube came out quite clean. Its contents had been reduced from 14.53 to 14.46, difference 0.07 grains, being 0.47 *per cent.* on the original carbonate, less than one two-hundredth part of the original weight, (exactly $\frac{1}{212}$). The chalk was in a state of firm saline marble, but with no unusual qualities.

The carbonate should be dried.

These two last experiments are rendered still more interesting, by another set which I made soon after, which shewed, that one essential precaution in a point of such nicety had been neglected, in not previously drying the carbonate. In several trials made in the latter end of the same month, I found, that chalk exposed to a heat above that of boiling water, but quite short of redness, lost 0.34 *per cent.*; and in another similar trial, 0.46 *per cent.* Now, this loss of weight equals within 0.01 *per cent.* the loss in the last-mentioned experiment, that being 0.47; and far surpasses that of the last but one, which was but 0.074. There is good reason, therefore, to believe, that had the carbonate, in these two last experiments, been previously dried, it would have been found during compression to have undergone no loss.

Explanation of some apparent anomalies.

The result of many of the experiments lately mentioned, seems fully to explain the perplexing discordance between my experiments with porcelain tubes, and those made in barrels of iron. With the porcelain tubes, I never could succeed in a heat above 28°, or even quite up to it; yet the results were often excellent. Whereas, the iron-barrels have currently stood firm in heats of 41° or 51°, and have reached even to 70°

or

or 80° without injury. At the same time, the results, even in those high heats, were often inferior, in point of fusion, to those obtained by low heats in porcelain. The reason of this now plainly appears. In the iron-barrels it has always been considered as necessary to use an air-tube, in consequence of which, some of the carbonic acid has been separated from the earthy basis by internal calcination: what carbonic acid remained, has been more forcibly attracted, according to Mr. Berthollet's principle, and, of course, more easily compressed, than when of quantity sufficient to saturate the lime: but, owing to the diminished quantity of the acid, the compound has become less fusible than in the natural state, and, of course, has undergone a higher heat with less effect. The introduction of water, by furnishing a reacting force, has produced a state of things similar to that in the porcelain tubes; the carbonate sustaining little or no loss of weight, and the compound retaining its fusibility in low heats*.

In the early part of 1804, some experiments were made with barrels, which I wished to try, with a view to another series of experiments. The results were too interesting to be passed over; for, though the carbonic acid in them was far from being completely constrained, they afforded fome of the finest examples I had obtained, of the fusion of the carbonate, and of its union with silex.

Experiment. Fusion of a portion of carbonate (from shell) and mutual attraction of the carbonate and silex of the tube.

On the 13th of February, an experiment was made with pounded oyster-shell, in a heat of 33°, without any water being introduced to assist compression. The loss was apparently of 12 *per cent.* The substance of the shell had evidently been in viscid fusion: it was porous, semitransparent, shining in surface and fracture; in most parts with the gloss of fusion, in

* The retentive power here ascribed to the porcelain tubes, seems not to accord with what was formerly mentioned, of the carbonic acid having been driven through the substance of the tube. But the loss by this means has probably been so small, that the native properties of the carbonate have not been sensibly changed. Or, perhaps, this penetrability may not be so universal as I have been induced to think, by having met with it in all the cases which I tried. In this doubt, I strenuously recommend a further examination of this subject to gentlemen who have easy access to such porcelains as that of Dresden or of Seve.

many others with facettes of crystallization. The little tube had been set with its muzzle upwards; over it, as usual, lay a fragment of porcelain, and on that a round mass of chalk. At the contact of the porcelain and the chalk, they had run together, and the chalk had been evidently in a very soft state; for, resting with its weight on the porcelain, this last had been pressed into the substance of the chalk, deeper than its own breadth, a rim of chalk being visible without the surface of the porcelain; just as when the round end of a knife is pressed upon a piece of soft butter. The carbonate had spread very much on the inside of the tube, and had risen round its lip, as some salts rise from their solution in water. In this manner, a small quantity of the carbonate had reached the outer tube, and had adhered to it. The black colour frequently mentioned as accompanying the union of the carbonates with the porcelain, is here very remarkable.

Similar experiment.

On the 26th of February, I made an experiment, in which the carbonate was not weighed, and no foreign substance was introduced to assist the compression. The temperature was 46°. The pyrometer had been affected by the contact of a piece of chalk, with which it had united; and some of the carbonate must have penetrated the substance of the pyrometer, since this last had visibly yielded to pressure, as appeared by a swelling near the contact. I observed in these experiments, that the carbonate had a powerful action on the tubes of Cornish clay, more than on the pounded silex. Perhaps it has a peculiar affinity for argil, and this may lead to important consequences. The chalk had visibly first shrunk upon itself, so as to be detached from the sides, and had then begun to run by successive portions, so as still to leave a pillar in the middle, very irregularly worn away; indicating a successive liquefaction, like that of ice, not the yielding of a mass softening all at once.

Similar experiment. Detail of appearances.

On the 28th of February, I made an experiment with oyster-shell unweighed, finely ground, and passed through the closest sieves. The pyrometer gave 40°. The piece of chalk below it had been so soft, as to sink to the depth of half an inch into the mouth of the iron air-tube, taking its impression completely. A small part of this lump was contaminated with iron, but the rest was in a fine state. The tube had a rent in

it,

it, through which the carbonate, united with the matter of the tube, had flowed in two or three places. The shell had shrunk upon itself, so as to stand detached from the sides, and bore very strong marks of fusion. The external surface was quite smooth, and shining like an enamel. The internal part consisted of a mixture of large bubbles and solid parts: the inside of the bubbles had a lustre much superior to that of the outside, and equal to that of glass. The general mass was semitransparent; but small parts were visible by the lens, which were completely transparent and colourless. In several places this smooth surface had crystallized, so as to present brilliant facettes, steadily shining in certain aspects. I observed one of these facettes on the inside of an air-bubble, in which it interrupted the spherical form as if the little sphere had been pressed inwards at that spot, by the contact of a plane surface. In some chalk near the mouth of the large tube, which lay upon a stratum of silex, another very interesting circumstance occured. Connected with its lower end, a substance was visible, which had undoubtedly resulted from the union of the carbonate with the silex. This substance was white and semitransparent, and bore the appearance of chalcedony. The mass of chalk having attached itself to that above it, had shrunk upwards, leaving an interval between it and the silex, and carrying some of the compound up with it. From thence this last had been in the act of dropping in a viscid state of fusion, as evidently appeared when the specimen was entire; having a stalactite and stalagmite corresponding accurately to each other. Unluckily I broke off the stalactite, but the stalagmite continues entire, in the form of a little cone. This new substance effervesced in acid, but not briskly. I watched its entire solution; a set of light clouds remained undissolved, and probably some jelly was formed; for I observed, that a series of air-bubbles remained in the form of the fragment, and moved together without any visible connection; thus seeming to indicate a chemical union between the silex and the carbonate The shell, fused in the experiment, dissolved entirely in the acid, with violent effervescence.

In the three last experiments, and in several others made at the same time, the carbonate had not been weighed; but no water being introduced to assist the compression, it is probable there was much loss by internal calcination; and owing doubtless

doubtless to this, the carbonates have crumbled almost entirely to dust, while the compounds which they had formed with silex remain entire.

Similar experiment. Resemblance to Cipolline marble.

On the 13th of March, I made a similar experiment, in which, besides some pounded oyster-shell, I introduced a mixture of chalk, with 10 *per cent.* of silex intermixed, and ground together in a mortar with water, in a state of cream, and then well dried. The contents of the tube when opened, were discharged with such violence, that the tube was broken to pieces; but I found a lump of chalk, then in a state of white marble, welded to the compound; which last, in its fracture, shewed that irregular black colour, interspersed roughly through a crystalline mass, that belongs to the alpine marbles, particularly to the kind called at Rome *Cipolline.* It was very hard and firm; I think unusually so. It effervesced constantly to the last atom, in diluted nitric acid, but much more sluggishly than the marble made of pure chalk. A cloudiness appeared pervading all the liquid. When the effervescence was over, a series of bubbles continued during the whole day in the acid, without any disposition to burst, or rise to the surface. After standing all next day and night, they maintained their station; and the solution being stirred, was found to be entirely agglutinated into a transparent jelly, breaking with sharp angles. This experiment affords a direct and positive proof of a chemical union having taken place between the carbonate and silex.

(To be continued.)

III.

Observations on the Dispersion of the Light of Lamps by Means of Shades of unpolished Glass, Silk, &c.; with a Description of a new Lamp. By BENJAMIN COUNT OF RUMFORD. *Read at the National Institute of France, March* 20, 1806.

To Mr. NICHOLSON.

SIR,

I SEND you for your Journal, the following translation of a very ingenious paper of Count Rumford's, which was read at

at the Institute on Monday last. Count Rumford, at whose desire I send it, has taken the trouble to revise and correct the translation.

I am,

Paris, Your very humble servant,

27th March, 1806. W. A. CADELL.

No. 17, *Rue de Varenne.*

Amongst the necessaries of life may be reckoned *heat* and *light*; and each of them composes so considerable an article of expence, that every improvement that tends to facilitate their production, or to economise their consumption, is deserving of attention.

Introduction.

Having made, at different periods, a great number of experiments on the production of light in the combustion of inflamable bodies, and on its distribution; and having lately contrived a lamp which, on trial, has been found to answer very well, I have resolved to submit to this learned assembly some of the results of my researches on this important subject.

An improved lamp,

The lamp, which I have the honour of presenting to the Institute, has nothing new in the essential part of it, that is, in the form of the wick; for, after the ingenious discovery of the circular wick by M. Argand, it does not appear to me probable that the economy of oil in the production of light can be carried much farther: When this lamp is in good order it gives no perceptible smoke, nor smell; and hence, I think, we may conclude that the combustion of the oil is complete, and, consequently, that the quantity of light is at its maximum. But there still remains much to be done to improve the general form of lamps, in regard to their elegance and convenience, and above all, to distribute their light in a more advantageous and agreeable manner.

—not essentially new.

If the facility with which the eye distinguishes objects depended solely on the intensity of the light by which they are illuminated, the scientific distribution of light would be less important; but that is far from being the case. We are able to see, very distinctly, with intensities of light which are extremely diffierent; provided that the eye has had the time to conform itself to the quantity of light present, and that that quantity remains invariable.

Vision is well performed by light, at very different intensities.

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It is well known that we can read printed characters of a moderate size, both by the light of the full moon, and by that of the sun at mid-day; the intensity, however, of the light, in the first case, is, to that in the second, as 1 to 300,000; but when the eye passes rapidly from a strong light, to one that is more feeble, or *vice versa*, we can distinguish nothing at first; and, when these changes succeed each other rapidly, they become extremely fatiguing to the eyes.

Distinctness depends very much on the simplicity of the shadows of bodies.

The facility with which we distinguish an enlightened object depends on its shadow. When shadows are simple, they are necessarily well defined, and we see distinctly; but when the light comes in several directions, several shadows are formed of the same object, which are so blended together as to render them confused and ill defined; and in that case we see indistinctly, even in the midst of a great glare of light. Hence we may conclude, that a considerable economy must result from a judicious distribution of the light employed in lighting a room. But this saving of expence, considerable as it would be, is however an object of much less importance than the advantage which must result in respect to the pleasantness of the light, and the preservation of the eyes.

The hurtful effects of the direct light of lamps upon the eye.

If every sudden change in the intensity of the light that falls upon the eyes be hurtful to them, the direct rays coming from the flame of an Argand lamp must fatigue them very much, and even deprive them of the faculty of distinguishing easily objects which are placed near that dazzling source of brightness. It is impossible indeed to view the flame of one of these lamps near at hand without suffering excessive pain, and even at a distance it is always hurtful and unpleasant to the eye. It is well known how much we are dazzled and almost blinded on coming into a room lighted by several of these lamps, burning without shades, and placed so low that the eye cannot avoid them.

—remedied by semitransparent shade.

With a view to soften the light of these lamps, shades have been contrived, formed of materials whose transparency is more or less imperfect; for instance, large cylinders, or spheres of crape, gauze, or roughened glass. This contrivance is very useful, and deserves to be more generally adopted; it is even of so great importance that we cannot take too much pains to improve it, and recommend it to the public.

What has hindered these shades from being more generally employed is probably an opinion, that they must necessarily occasion a great loss of light. I hope to be able to shew that that opinion is not well founded.

The use of shades has been limited from a notion that they darken;

The following simple experiment was made some years ago, with a view to determine nearly the quantity of light which is lost in passing through a roughened glass.

Two wax candles, of equal size, lighted, and burning with the same degree of intensity, were placed in two vertical cylinders of fine glass, pretty thin, six inches in diameter and six inches in height, the one of smooth and the other of roughened glass; these two cylinders being placed at the same height, on two tables, at the distance of eight feet from each other, in a room where there was no other light than that emitted by the candles, I presented to the two candles, placed in their cylinders, a sheet of white paper, at the distance of sixteen feet from each of them, and I interposed before the paper, at the distance of two inches from its surface, a small wooden cylinder in a vertical position, which projected two shadows on the paper.

but experiment shews that roughened glass intercepts no more light than clear glass.

I was much surprised to find these shadows very nearly of the same intensity. This result shewed me that the quantity of light lost in passing through a roughened glass is much less than I at first supposed; but, on reflection, I saw that there was nothing in the result of the experiment which did not admit of an easy explanation.

The cause is manifest;

Although roughened glass appears opaque, it is by no means so. In the operation of roughening its surface, which from being smooth becomes furrowed, and broken in every direction, it at last presents an uninterrupted collection of asperities of every different form. Individually they are almost invisible to the naked eye, on account of their smallness; their sides are however smooth and shining, as is easy to perceive on examining them with a microscope. It is evident that the light which falls upon the smooth surface of one of these little prominent points must penetrate the glass with the same ease (when the angle of incidence is the same) as it would penetrate the plane surface of a large polished plate of the same sort of glass; and that having passed through the surface, the ray must pursue its course in the substance of the glass, and pass out on the other side in the same manner in one case as in the other.

for the minute cavities of rough glass are polished.

These small surfaces only disperse the rays.

When a pencil of paralell rays falls perpendicularly upon a plate of well-polished glass, they pass through the glass without any perceptible change of direction; but when the pencil falls upon a plate of roughened glass, the rays of which it is composed are dispersed, and the cylindrical pencil is transformed into a cone. The ultimate course of each ray depends on the refractions that it has undergone in entering and issuing out of the glass, and these refractions are determined by the angles of incidence, and the respective inclinations of the refracting surfaces on each side of the glass at the point where the ray enters and at that where it goes out.

A clear glass globe surrounding a lamp is scarcely seen; if the glass be roughened it emits light from every part.

If the flame of a lamp be placed in the center of a globe of fine glass, well polished, the rays issuing from it will traverse the sides of the globe without undergoing any perceptible change, either in their intensity or their direction; and the flame will be seen so distinctly through the globe that this last might even escape observation. But if, instead of a globe of polished glass we employ a globe of roughened glass, in that case, the rays emitted by the flame will be dispersed by the glass in such a manner that each visible point of the surface of the globe will become a radiant cone, and consequently the globe will appear luminous, diffusing light from its surface in every direction.

The light is thus softened and very little lost,

From this explanation of the phenomena we see that a shade of fine glass roughened, when it is used with a view to disperse and soften the too vivid light of a lamp, does not occasion any considerable loss of light. This loss would even be imperceptible, or not greater with a shade of roughened glass than with one of the same kind of glass polished and transparent, notwithstanding the great dispersion of the light, were it not for the reflections which some of the rays suffer before they quit the shade.

by internal reflections.

It is well known that when a ray of light falls upon a polished surface of glass (or other substance), at a very small angle of incidence, it is necessarily reflected; and as the sides of the asperities of the roughened glass must present themselves to the rays which proceed from the lamp at angles of all possible magnitudes, there must necessarily be some whose inclination is sufficient to reflect some of the rays that fall upon them; and as that may occur at both surfaces of the shade, it

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is possible that a ray may be obliged to pass and repass in the glass from one side of the shade to the other several times before it be able to escape into the room.

If the glass were perfectly transparent, the light would be little, or perhaps not at all diminished by these repeated reflections and passages; but we know that even the finest glass is very far from being perfectly transparent.

When crape, gauze, or other substances are employed to make shades for the purpose of masking the flame of a lamp, the loss of light will be more or less considerable in proportion to the greater or less degree of transparency of the solid parts of the substance employed. But without engaging in the very delicate enquiry concerning the degree of transparence of the molecules or small solid particles of the substances to be employed in making shades, we may determine, by simple experiments, with ease and even precision, what are the substances to be preferred for that purpose. We have only to procure shades of the same form and size, made of the different substances to be examined, and to compare them, by pairs, by means of two argand lamps, made to burn with the same degree of vivacity, and of a simple photometer, which can be constructed at a very small expence.

Shades of crape, gauze, &c. will intercept light in proportion to their opacity.

The photometer which I used in my experiments on the comparative quantities of light produced in the combustion of wax, tallow, and different kinds of oil, and of the same kind of oil burned in an argand lamp and in a common lamp,* would serve perfectly well for the experiments in question; but as that instrument is somewhat complicated, I shall propose another more simple, which I have employed since with success. Its construction is as follows:—

The loss may be ascertained by the photometer,

In the middle of the upper surface of a wooden cube of 8 inches in diameter, composed of boards, covered with black paper, there is fixed vertically a small board, 4 inches in breadth, 6 inches in height, and half an inch in thickness, covered on one side with white paper. In the middle of this white side there is traced, with pen and ink, a slender black line, from the top to the bottom, which divides the surface into two equal parts.

or by a simpler instrument here described.

* See Phil. Transact. for 1794, and my Philosophical Papers, vol. i. page 270.

Before this white surface, at the distance of $2\frac{4}{10}$ inches, are placed two little pillars of wood, painted black, 4 inches in height and half an inch in diameter. These little pillars, which are cylindrical, are placed at the distance of $3\frac{2}{10}$ inches asunder, and they are firmly fixed in two holes formed to receive them in the upper surface of the cube. They are at equal distances from the black vertical line which marks the middle of the white surface of the photometer, that is to say, at the distance of 3 inches (English measure) from that vertical line.

ethod of em-
ying that
pparatus.

This little instrument is employed in the following manner: Having placed, in a dark room, three little tables, at the distance of 7 or 8 feet from each other, so as to occupy the three angles of an equilateral triangle, the photometer is placed on one of these tables, and the two lamps upon the other two; taking care that the flames of the lamps and the middle of the white surface of the photometer are of the same height, or in the same horizontal plane.

The observer being seated before the photometer, with his back turned towards the lamps, he presents the photometer to the two lamps, in such a manner that the direct rays from their flames fall upon the white surface of that instrument at equal angles of incidence, or in such a direction that the two internal shadows formed by the two pillars may touch each other without being blended together, at the black vertical line in the middle of that face. As the two external shadows fall without the surface of the photometer, they are of course not seen.

When the photometer is placed, the distances of the lamps are verified, and brought to a perfect equality, and when that is done, the lamps are made to burn with the same degree of vivacity, which is easily done by elevating a little one of the wicks, or lowering the other: this must be performed by an assistant, whilst the observer keeps his eyes constantly fixed upon the shadows.

The equality of the quantities of light which the lamps emit is announced by the perfect equality in the densities of the two shadows which are formed in the middle of the white face of the photometer. This is evident, because each shadow being enlightened by the direct rays of the opposite lamp, if one of the lamps gives more light than the other, the shadow which it enlightens must of course be more enlightened, and con-

sequently

sequently less dark than that enlightened by the weaker lamp.

If, instead of establishing an equality in the quantity of light emitted by the two lamps, we would ascertain the relative quantities of light that they emit when their flames are unequal, they must be placed on two tables before the photometer, and after having brought the shadows into contact with each other, we must remove the stronger lamp until the intensity of its light at the vertical field of the photometer be diminished by the increase of its distance, till a perfect equality is established between the densities of the two shadows; and then we measure exactly the distance of each lamp from the photometer.—The squares of the distances will be as the quantities of light emitted by the lamps.

In order to exclude the light reflected from the sides of the room and other surrounding bodies, with a view to render the shadows more distinct, and their comparison more easy, the photometer may be placed in a quadrangular box, open in the front, forming a kind of centry-box, 15 or 16 inches in height and 10 or 12 inches in width and depth, constructed of boards, or even of pasteboard, and lined within and without with black paper.

Valuable results may be had by experiments upon shades.

The experiments are simple and easy that may be made with this little apparatus for determining the different substances that may be employed in constructing shades to soften the light of lamps; and as this enquiry must lead to very important improvements, both with respect to economy and to elegance and comfort, I recommend these researches to all those who are occupied in the improvement of lamps. These experiments may be made in the following way:—

Instructions for making such experiments.

Having prepared two shades, of the same form and dimensions, that are to be compared together, we must begin by placing the two lamps at the same distance in front of the photometer, and causing them to burn with the same degree of brightness, and then masking the flames of the lamps by the two shades, we must examine anew the shadows. If these shadows are of equal densities, we may conclude that the two shades emit equal quantities of light; if the densities of the shadows are different, then the shade that enlightens the shadow which is the least dense is that which emits the most light;

and

and in order to determine with precision the relative quantities of light emitted by the two shades, we must remove the lamp which bears the shade that emits the most light until the equality of the shadows be re-established, and then, measuring the distances of the lamps from the photometer, the quantities of light will be as the squares of these distances.

If we wish to know how much light is absorbed and lost in employing any given shade, we must operate in the following manner:—Having placed the two lamps, without their shades, at equal distances in front of the photometer, and having equalised the flames of the lamps in the manner already described, we place the shade that is to be tried upon one of the lamps, and the equality of the shadows is instantly destroyed. In order to re-establish this equality, we remove the lamp that is without a shade, and when it re-established we measure the distances of the two lamps from the photometer. The quantity of light emitted by one of these lamps without a shade is to that emitted by the same lamp with the shade, as the square of the distance of the lamp that burns without a shade is to the square of the distance of that masked by the shade.

The power of a shade to soften and disperse light, and that of intercepting light, are different qualities.

The object in view in using a shade being to disperse the rays of a too dazzling flame, without destroying them, it is evident that the less the flame of a lamp is apparent through the sides of a shade, the quantity of light emitted being the same, the better it will answer its purpose. But as we always see, more or less distinctly, the brilliant flame of an argand's lamp through the shade which masks it, it is evident that a considerable part of the light emitted by a lamp thus masked does not proceed from the shade, but, passing directly through the sides of the shade, it comes from the flame in straight lines.

It is light, coming from the flame to the eyes in straight lines, which a shade is destined to disperse and to soften; and as it is certain that two shades of different materials may have an equal power in softening the direct rays of the flame of a lamp, and that nevertheless the total quantities of light that they emit may be very different, it is necessary to pay attention to this remarkable circumstance in the choice of shades.

The shades to be compared should therefore be examined, first in regard to their power of masking and softening the direct rays of the flame of a lamp, and afterwards in regard to the

the quantity of light that they distribute in a room. The first point seems susceptible of being pretty well determined by the simple inspection; but if we would employ more precision, we may make use of the following method :—

Instructions for ascertaining the power of dispersing or softening the light,

Having placed, at equal distances, two lamps burning with the same degree of vivacity, in front of the photometer, we mask them with the two shades (made of different materials), which are to be compared, and we place between each shade and the photometer, at the distance of about an inch from the shade, a disk of thick pasteboard, perforated in the middle by a circular hole, one inch in diameter. The diameter of this disk must be sufficiently large to mask completely the shade, and the center of the circular hole must be in a straight line drawn from the centre of the flame to the center of the vertical face of the photometer.

and of generally illuminating.

We see that, in this state of things, it is almost solely the direct rays coming from the flames of the lamps in straight lines through the substance of the shades that fall upon the photometer; and that, on measuring the relative intensities of these rays on each side, by means of the shadows and the distances, we can determine not only which of the shades fulfils best its principal object, that of preserving the eyes, but likewise the proportion in which one of the flames is more softened than the other. We may also determine, by easy experiments and calculations, the proportion which exists, in any given case, between the quantity of light that passes directly from the flame in straight lines through the sides of the shade, and that which is dispersed by the shade and appears to issue from the shade itself. It would be too long to describe in this place all these experiments, and several others that might be made with the photometer, to complete the researches necessary for the improvement of lamps; but these details are the less necessary, as the experiments will not fail to present themselves to those who shall have made some progress in these enquiries.

Concerning the size of shades.

I shall finish my observations upon shades for lamps by some remarks on the size that may be given to them. And first, it is evident that the diameter of a shade should be greater in proportion as the flame which it is intended to mask is greater and more bright; for if a shade be small, the light which

which it emits may be sufficiently strong to hurt the eyes, especially when viewed at a small distance.

The size and intensity of the flame being the same, the intensity of the light emitted by the surface of a shade that masks it will be as that surface, and consequently as the square of the diameter of the shade inversely.

The larger the shade the less luminous it will appear when looked at.

If the intensity of the light emitted by a shade four inches in diameter be equal to four, it will be reduced to one on doubling the diameter of the shade, and that without any change in the total quantity of light which is diffused in the room. This shews the advantage to the eyes that will result from the use of shades of large dimensions.

Small shades appear dazzling.

The small spherical shades of roughened glass which are sometimes employed for lamps, have been found to emit a light too dazzling to the eyes. In order to remedy this inconvenience, all that is necessary is to make the shade larger. If these globes are more dazzling than globes of crape or gauze of the same dimensions, that circumstance proves no more than that roughened glass absorbs less light than these silk stuffs do; and from this we may conclude that the solid parts of silk are less transparent than those of glass, and consequently that this substance is less fit to be used in making shades for lamps than glass.

Great advantage of glazing windows with roughened glass when the illumination is very obliquely received, as in courts, alleys, &c.

I will just mention here a circumstance respecting roughened glass, which although not immediately connected with the subject of this paper, appears nevertheless sufficiently important to deserve attention. It often happens, in great towns, that a room has no other light than what it receives by windows which look into a small court, surrounded on all sides by high buildings: in these cases the room would be more copiously and better lighted by panes of roughened glass than by transparent panes. The rays of day-light, which descend almost perpendicularly from above into the court, fall upon the panes at so small an angle of incidence that, when the exterior surface of the glass is polished, they are in great part thrown off by reflection, and do not get into the room; and even those which, not being reflected, pass through the pane, as they fall directly upon the floor, where they are almost all absorbed, the objects in the room are very little enlightened; but when the pane is roughened, the asperities of the

the glass presenting to the descending rays surfaces less inclined, more of the rays enter the glass, and passing through it afterwards in various directions, light is diffused in all the parts of the room. And it is not solely for windows which look into small courts that it is useful to employ roughened glass; it may be used with much advantage in all cases where windows look against a high wall, at a small distance, and especially if the wall be of a dark colour. But to return to my subject.

Without enlarging farther at this time on the construction of shades to be used for masking and softening the too dazzling flame of lamps, I shall give an account of the new lamp which I have lately caused to be constructed, and which I have the honour of presenting to this learned society. Account of the lamp.

This lamp, which is destined to be suspended in the middle of a room, was particularly intended to light a dining-room, but it may likewise serve to light a drawing-room, or a billiard-table. The following are the details of its construction:—

A hollow hoop of tin, painted white, $12\frac{8}{10}$ English inches in diameter within, 16 inches in diameter without, and $\frac{8}{10}$ of an inch deep, suspended in a horizontal position, serves as a reservoir for the oil. In the centre of this circular reservoir there are three cylinders, or beaks, which inclose three circular wicks of the usual form and size. These three vertical cylinders, which touch each other, are soldered together, and connected with the reservoir by means of three oblique tubes $\frac{2}{10}$ of an inch square, through which the oil flows to the wicks from the reservoir.

In order to catch the oil which occasionally drops from these three cylinders, there is a cup, made of tin $4\frac{1}{2}$ inches in diameter above at its opening, and 1 inch in depth in the middle, which is placed at the distance of $\frac{3}{4}$ of an inch under the lower ends of the three cylinders.

Each of these cylinders is furnished with a chimney, or tube of glass, and they may be lighted either all three together, or two, or one only, according to the quantity of light that is wanted.

This lamp is suspended by means of a hoop of brass, gilt, $16\frac{2}{10}$ inches in diameter and $1\frac{1}{4}$ inch wide, having a little horizontal projection at its lower edge, internally, on which

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Description of a lamp by Count Rumford.

the circular reservoir of the lamp rests. To this brass hoop are fixed three arrows, of gilt brass, at equal distances from each other. These arrows, which are 6 inches in length and $\frac{1}{10}$ of an inch in diameter, are in a horizontal position, on the outer side of the hoop, and in the direction of three radii drawn from its centre.

To these three arrows, at the distance of 3 inches from the hoop, are fastened the ends of three chains of gilt copper, each of 28 inches in length, by which the hoop that receives the lamp is suspended.

These arrows serve the purpose of separating the chains from each other, in order that the lamp may be removed occasionally, and replaced, without deranging the chains.

For a lamp with four wicks, which serves to light a large drawing-room, the gilded hoop that receives the lamp has six arrows, to which are attached six chains; but in order to be able to remove and replace the lamp, there is one of the chains which, being attached to its arrow by a small hook, it is detached occasionally and laid aside, in order to allow a passage for the lamp.

The gilded hoop which receives the lamp is ornamented with pendants of cristal; and from the lower edge of the hoop, immediately behind the cristal ornaments, there descends a hoop of white crape, of the same diameter as the hoop of brass, and $4\frac{1}{2}$ inches in breadth, which serves to disperse and soften the direct rays of the flames of the lamp.

To reflect a part of the rays that mount towards the ceiling, in order to destroy the shadows that might be formed under the lamp, there is a conical reflector of white crape, which, resting on the three tubes that conduct the oil from the reservoir to the wicks, surrounds and conceals the tubes of glass that contain the flames.

This reflector is $12\frac{1}{2}$ inches in diameter below, $5\frac{1}{4}$ inches in diameter at its opening above, and 6 inches high.

The chief difficulty to be overcome in the construction of this lamp was to contain the oil in the reservoir in such a manner that it should not be in danger of being spilled in taking the lamp out of the hoop, in which it is suspended, in carrying it from one place to another, and in replacing it. Several attempts had already been made, by different persons, to construct

struct suspended lamps with circular horizontal reservoirs, but none of them had been attended with success. That which I now propose is simple in its construction, and appears to me to answer perfectly well.

Description of a lamp by Count Rumford.

This reservoir is closed above, so as to form a hollow hoop, and it has three openings on its upper surface, equidistant from each other. These openings, which serve for pouring the oil into the reservoir, have each $\frac{7}{10}$ of an inch in diameter, and are hermetically closed by three stoppers of brass, ground with emery. In the axis of each of these stoppers there is a small hole, $\frac{2}{10}$ of an inch in diameter, which is occasionally closed by a small screw furnished with a collar of leather.

When the reservoir is filled with oil, the three stoppers are put in their places, and the small holes are then closed by means of the three screws. In this state of things, as the air cannot enter into the reservoir by the opening at its upper surface, the lamp may be transported, and even inclined considerably, without any danger of spilling the oil. As soon as the lamp is placed in its hoop (where care is taken to suspend it in a horizontal position), the communication must be opened between the air of the atmosphere and the upper surface of the oil in the reservoir, which is done by unscrewing a few turns the small screws which are in the axis of the stoppers. The oil then resumes its natural level, and afterwards passes freely into the cylinders that contain the wicks in order to feed the flames.

That it may not be necessary to take out the screw entirely when a passage for the air is opened by means of the vertical holes in the axis of the stoppers, these screws, which are half an inch in length, are not complete, being reduced on one side to the half of their diameters, in their whole length, except about $\frac{1}{10}$ of an inch at the top near the collar of leather.

When the screw is unscrewed two turns, the part of the screw that still remains in the hole not being entire, a free passage is necessarily opened into the interior of the reservoir.

It would be possible to fill the reservoir of this lamp by one opening only, which would require only one stopper and one screw; but I have found by experience that it is inconvenient, and that it is much better to fill the reservoir by three holes, in the manner above described; for in that case the air gets

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Description of a lamp by Count Rumford.

out of the reservoir easily, and the oil enters without any obstacle.

When this lamp is filled with oil the reservoir must be firmly placed in a horizontal situation, upon a stand made on purpose to support it during the operation, and the three stoppers must be taken out; the reservoir being filled, care is taken to replace the stoppers, and to close the little holes by the screws before it is taken from its stand.

These little holes must not be opened till the lamp is suspended in its place in the gilt hoop, and at rest; and attention should always be paid to close them before lifting the lamp to take it from its hoop. These precautions are absolutely necessary in order to avoid the risk of spilling the oil.

When this lamp is suspended at a proper elevation above the middle of a round table, large enough for placing conveniently ten or twelve persons, in a room 24 feet long, by 20 feet wide, and 15 high, not only the table, but likewise the whole room is completely lighted, without the least visible shadow being produced in any part, and without any person at table, or in the other parts of the room, being incommoded by the direct rays from the three flames which are united at the centre of the lamp. The diameter of the hoop of crape which masks these flames is so great that the light which it emits from its surface is very soft, although it receives the direct rays of the three flames.

As the light which this lamp diffuses in a room proceeds from one single source, the shadows of the enlightened objects are of course simple, and well defined, a circumstance which certainly contributes much to the ease with which we distinguish objects, as well as to the pleasantness of the illumination, and the preservation of the eyes.

In order to light the table of a dining-room or study, of 5 or 6 feet in diameter, a small lamp with one wick will be sufficient, and instead of suspending it from the cieling, it may be placed on a pedestal, at the height of 12 or 15 inches above the table. For a lamp with one wick, intended to burn 8 or 10 hours, the circular reservoir for the oil may be made 6 inches in diameter within, 1 inch wide, and $\frac{6}{10}$ of an inch deep. The conical reflector of crape, or roughened glass, for this lamp should be 8 inches in diameter below, 2 inches in diameter

meter at the top at its opening, and 5 inches in height. The hoop of crape which surrounds the lamp should be 8 inches in diameter and $3\frac{1}{2}$ inches in breadth. If it be desired to have more light upon the table, and less in the room, the conical reflector which covers the lamp above may be made of a thicker stuff, or even of tin, painted white within, and painted and varnished without.

Description of a lamp by Count Rumford.

To light a large drawing-room, or dining-room, a suspended lamp with six wicks may be used, with a reservoir of oil 18 inches in diameter within, $2\frac{1}{2}$ inches in breadth, and $\frac{8}{10}$ of an inch in depth, surrounded by a hoop of crape 6 inches broad.

Such a lamp, hung at the height of 8 or 9 feet in the middle of a large room, would be found to diffuse a very gentle and agreeable light.

It is almost superfluous to observe, that the general form of this lamp is simple and elegant; and that it is susceptible of being easily ornamented, a circumstance which is of real importance in this age of refinement, and even in every age in which the sentiment of beauty has any influence on the manners and happiness of mankind.

N.B. The openings of the tubes above, which contain the wicks, are situated one quarter of an inch above the level of the oil in the reservoir, when the reservoir is full.

PLATE I.

Fig. 1.—Horizontal projection.

d. Hoop that serves for reservoir of oil.

e. Tubes that convey the oil from the reservoir to the cylinders.

f. Cylinders.

a. Brass stoppers, with their screws.

g. Brass hoop which serves to receive the lamp.

h. Arrows attached to the brass hoop: to these arrows the chains are fixed.

Fig. 2.—Vertical projection.

d. Hollow hoop that serves for reservoir of oil.

e. Tubes that convey the oil from the reservoir to the cylinders.

f. Cylinders, with their chimneys.

a. Brass stoppers, with their screws.

g. Brass

g. Brass hoop which serves to receive the lamp.
h. Arrows attached to the brass hoop.
i. Cup that receives the oil which may fall from the cylinders.
k. Cristal pendants attached to the brass hoop.
l. Hoop of white crape attached to the lower edge of the brass hoop immediately behind the pendants.
m. Reflector of white crape, which rests on the tubes e.

Fig. 3.—On a larger scale.

a. Section of the brass stopper with the small screw in its axis: the screw is represented as open to give admission to the air into the reservoir.
b. Collar of leather which, when the screw is closed, presses upon c, and excludes the air of the atmosphere from the reservoir.
d. Section of a part of the hollow hoop which serves as a reservoir for the oil.

IV.

Facts towards a History of Tin. By Professor PROUST.*

Tin and Muriate of Ammonia.

Facts and observations on tin and its compounds.

THE muriate when heated with granulated tin yielded the following results:—

When the heat is on the point of evaporating the muriate, the tin acts upon the water of this salt, and decomposes it. The metal seizes its oxigen, and causes a disengagement of inflammable gas. One hundred ounces of muriate afford from eleven to twelve inches of gas: they may perhaps afford more; but the retort generally bursts before the operation is completed. This hydrogen has nothing in it remarkable.

At the close of the operation, a saline mass is found, composed of muriates of tin and ammonia, and of the granulated tin. The oxide of this muriate is only at the minimum; for it gives a purple colour with gold, and black with hydrosulphurated water, &c. If tin be merely boiled in a solution of sal ammoniac, a considerable portion will be dissolved.

* Journal de Physique, vol. lxi. p. 338.

Mosaic

Mosaïc Gold.

Facts and observations on tin and its compounds.

If a mixture of tin, sal ammoniac, and sulphur be heated together, it happens at the moment the latter is decomposed that the muriate of tin, which is formed, seizes part of its oxide, and transforms it into mosaïç gold. But as the ammoniac, in proportion as it is set at liberty, saturates the new products, and disguises the results, we shall resume this experiment in a different manner.

Muriate of Tin and Sulphur.

The muriate is first to be concentrated to the minimum, even till it congeals. The aqueous product of this by distillation contains muriate at the maximum. This happens because the latter is more volatile than the muriate at the minimum. Distillation is therefore a mean whereby the minor muriate may be purified from that whose oxidation has been increased by the atmosphere. The minor muriate is also volatile; but it requires a much higher temperature than the other: this is demonstrable by the process for making the fuming liquor. The muriate at the maximum is raised by a very gentle heat; whilst the muriate at a minimum remains in the retort: this is their difference.

Flowers of Sulphur are next to be cast into the congealed muriate, and heat gradually applied. In a few seconds volatile fuming muriate will pass over in considerable quantity. The excess of sulphur will fix about the neck of the retort, and at the bottom will appear a light mass of brilliant mosaïc gold, and part spangle the dome of the vessel with gold-coloured flowers.

On an attentive examination of these products, it will be discovered,—

1, That the muriate of tin is divided into two parts.

2, That one of these is deprived of all its acid in favour of the other.

3, That it also parts with some of its oxigen, which raises the other to the quality of fuming volatile muriate.

4, That tin oxided to the minimum, combines with sulphur only in proportion to a certain reduction, which takes place in the quantity of its oxigen: this reduction may henceforth be considered as subject to a fixed degree, in common with

Facts and observations on tin and its compounds.

with all those which are determined by the affinities in general. The proof of this may be observed in the constant properties of mosaïc gold: it exhibits a crystallisable volatile combination, as invariable in the whole of its characters as cinabar is, whatever may be the process from which it is obtained.

The following experiment offers a second proof of the diminution of oxidation, which takes place in the oxide at the minimum, before the production of mosaïc gold:—

Fifty parts of sulphur and one hundred of the grey oxide, or oxide at a minimum, deprived of water by a slight calcination, were heated in a defective retort. A moment arrived at which the mixture, though still at a very low temperature, entered into incandescence, and suddenly presented that phenomenon of ignition which is common to most metals when they combine with sulphur. After this appearance, the heat was raised till the mixture was faintly ignited, and it was continued till all the excess of sulphur was condensed in the neck of the retort. When cold, it was weighed, and was found to have lost 8 or 9 parts of its weight. The mosaïc gold remaining in the vessel weighed 120 or 121 parts.

Let us now examine these results:—

Sulphureous gas escapes from the retort, which accounts for the 8 or 9 parts lost of the weight; for nothing else escapes, as the excess of sulphur remains in the neck of the retort. Hence, if there were no formation of sulphureous acid, and consequently no loss of oxigen, the mosaïc gold obtained would be composed of 100 parts oxide + 20 of sulphur. But there is a deduction of oxigen: the mosaïc gold is therefore composed of oxide 100, — an unknown quantity of oxigen, 20 of sulphur, + a quantity of sulphur equal to that unknown quantity of oxigen. Mosaïc gold, therefore, is not a sulphurated oxide, in the degree hitherto imagined; or, in other words, a combination of sulphur with one or other of the two oxides of tin with which we are acquainted; but it is a sulphuret, whose oxide is fixed at a degree inferior to their constituting the minimum of oxidation of this metal: a constant term, I repeat; because, whenever the attributes of a compound present themselves without variation, whatever may be the process by which it has been obtained, the invariability in the proportion of its parts is always an inseparable consequence.

It

It remains, therefore, to ascertain what may be this new degree of oxidation, produced by affinity, exclusively to give existence to a singular combination, and to discover if it be capable of being exhibited separately, like those which we know do form the maximum and minimum of tin. I shall conclude this paragraph by observing that three operations, repeated with care, agreed to nearly half a grain in giving similar results.

Facts and observations on tin and its compounds.

The oxide at a maximum heated with sulphur produced an abundance of sulphureous gas, leaving mosaïc gold as a residuum. In this approximation, therefore, the metal abandoned all the oxigen comprised between 28 *per cent.* and the new term, inferior at 22, which we have just discovered.

If mosaïc gold be heated in a high temperature, the oxigen separates from the metal, combines with the sulphur, and escapes in sulphureous gas: but a part of the sulphur is retained from the oxigen by the metal itself: and the products are thus metallic sulphuret, sulphuric gas, and sulphuret of tin. Such are the new binary combinations produced by the ternary union of mosaïc gold, when urged by a strong temperature.

Bergmann, and after him Pelletier, were well convinced that mosaïc gold required for its formation a greater quantity of sulphur than the simple metallic sulphuret. For, besides the sulphureous gas already mentioned, a portion escapes entire on heating the mosaïc gold. It is a curious fact, that this metal, whose affinity to sulphur might be expected to decrease in proportion to the quantity of oxigen it contains, should be capable of attracting a much larger portion than pure tin.

If three parts of oxide at a maximum, and one part of mosaïc gold be made red-hot, the latter will be decomposed. The sulphur contributes to disoxidate parts of the oxide; sulphureous gas is afforded, and after the operation a grey powder is found, being a mixture of oxide at a minimum, metallic sulphuret, and white oxide. Muriatic acid dissolves the grey oxide, and the metallic sulphuret with this produces sulphurated hydrogen. The oxide at a maximum, being much less soluble, is the last to dissolve. After decanting and adding fresh acid, this second solution differs from the foregoing in giving a yellow colour to hydro-sulpurated water, whilst the former gave a deep brown.

Pelletier, who observed so acutely, has suffered himself to be

Facts and observations on tin and its compounds.

be imposed on by some appearance with which I am unacquainted. He says that sulphuret of tin and cinnabar heated together, yield mosaïc gold. A result so contrary to principles, appeared incredible; I repeated the experiment, and found, that these two sulphurets heated, produced merely cinabar and sulphuret of tin; the one volatilised, the other moulded in the bottom of the retort.

All these facts sufficiently acquaint us with what takes place in the operation of converting tin into mosaïc gold. It would be useless to urge that the intervention of mercury is as superfluous in this preparation, as in that of fuming muriate of tin, as I have shewn, in 1801, in the "*Journal de Physique*," vol. lii.

Mosaïc Gold and Acids.

Sulphuret of tin is composed of metal 100, of sulphur 20. Of this Sage and Bergmann were assured: I also found the same proportions. Muriatic acid readily acts upon this sulphuret of tin at a minimum, sulphurated hydrogen, &c. But it is a singular circumstance that the same acid has not the least influence upon mosaïc gold; it merely clears it of metallic sulphuret, as has been remarked by Pelletier.

Nitric acid, which likewise easily destroys sulphuret, has as little power over mosaic gold: a fact not less extraordinary, when we recollect the facility with which tin and sulphur, under other circumstances, are acted upon by nitric acid.

To dissolve mosaïc gold, aqua-regia must be used, and it must have a long and continued boiling. The result is a kind of sulphate of tin at a maximum. It is decomposed by the heat, and after drawing over oil of vitriol, a residuum is obtained of spongy white oxide, which must be washed to cleanse it from acid. The edulcorating water contains not an atom of tin; sulphureous hydrogen discovers nothing in it, unless it be atoms of mercury, when the mosaïc gold of commerce has been used, arising from the small quantity of cinnabar sometimes found in it.

One hundred grains of saltpetre, and fifty of mosaïc gold, heated gradually in a small retort, exploded with much violence, and had nearly been attended with serious consequences to me.

Sul-

Facts and observations on tin and its compounds.

Sulphuret and Potash.

Liquid potash has not the least action upon sulphuret of tin; that of antimony is affected quite otherwise under similar circumstances. Antimony, however, is far from possessing so great an affinity to oxigen as tin does. This diversity shews how cautious we ought to be in forming previous judgments in chemistry.

Mosaïc Gold and Potash.

Liquid potash, assisted by heat, dissolves mosaïc gold. The changes it undergoes are curious. As they tend to throw a light on the theory of oxidation, it may be useful to detail them; but to do this with clearness, it is indispensable, first, to speak of the hydro-sulphurets of tin, combinations which I have hitherto but imperfectly understood, and of which the denomination will stand in need of being improved.

Major Hydro-Sulphuret of Tin.

A current of sulphurated hydrogen is passed through any solution, of which the oxide is perfectly at a maximum; and a yellow precipitate will be obtained, to be collected, washed, and left to dry. To obtain more precipitate, it is proper to saturate the excess of acid in the solution; for when that predominates too much, the hydrogen with more difficulty attracts the oxide.

The precipitate possesses the following characters: Heated with marine acid, it dissolves with effervescence, yields abundance of sulphurated hydrogen, and is reduced to a simple muriatic solution, in which the oxide is always found at a maximum. This clear yellow precipitate, so long as it remains clear, is what we call *hydro-sulphuret major;* it augments the number of those combinations, which serve, in chemistry, to demonstrate the facility with which mere heat can vary the affinities. At an ordinary temperature, sulphurated hydrogen is an acid which takes the oxide from muriatic acid: but at the temperature of boiling water, the latter, in its turn, acts upon the sulphurated hydrogen, and resumes its oxide of tin.

Dry hydro-sulphuret of tin is of a dark-brown colour: it is vitreous in the fracture of its pieces, as are likewise the oxide major, the purple of cassius, and the native oxide. Potash readily dissolves it, and acids precipitate it without alteration.

Facts and observations on tin and its compounds.

If it be gradually heated, it affords water of new formation, and gives out sulphureous gas, a little free sulphur, and a residuum of very beautiful mosaïc gold.

These latter products distinctly shew, that the hydro-sulphuret cannot sustain a high temperature without a tendency to become simplified: that the tin, for example, communicates oxigen to the two principles of sulphurated hydrogen, retaining only such a proportion as the affinities render necessary for the new combination in which it becomes mosaïc gold; and, lastly, if the temperature be augmented, the mosaïc gold, abandoning this oxigen, passes into the state of metallic sulphuret, a combination still more simple than mosaïc gold.

Minor Hydro-Sulphuret of Tin.

If a saturated solution of tin at the minimum be treated in the way already described, a powder is obtained of the colour of coffee, or a little darker, which is to be washed in boiling water. This slight heat augmenting the attraction of the particles, enables the hydro-sulphuret to resist the action of the air, which otherwise is apt to change it from brown to yellow, even whilst on the filtre, that is to say, from minimum to maximum.

This hydro-sulphuret is distinguished from the preceding by the following qualities:

It is black, or appears so; will not dissolve in potash without changing its state; and yields no mosaïc gold by heat.

It possesses, in common with the foregoing, the property of dissolving with effervescence, of restoring the gas which saturated its base; and, consequently, of giving muriate of tin at a minimum, if muriatic acid be used.

If this hydro-sulphuret, when fresh, be heated with potash, it divides in two; one part of its base gives to the other all its oxigen, and is thus reduced to the state of simple metallic sulphuret. In this state it is collected together at the bottom of the vessel. The other part, raised by this addition to the maximum, attracts also the sulphurated hydrogen of the former, and thus becomes hydro-sulphuret major. The metallic sulphuret being thus separated, a yellow powder is precipitated by acids from the liquor, possessing all the characters described in the hydro-sulphuret of tin at a maximum. I have observed, in speaking of antimony, that its hydro-sulphuret, or kermes, treated with potash, can also yield sulphuret of antimony.

Black

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Black hydro-sulphuret, heated in a retort, gave abundance of water and a little free sulphur, but no sulphureous gas, and was reduced to a pure and simple metallic sulphuret; that is to say, although the quantity of tin in this hydro-sulphuret be as 22 to 100, it does not stop at this inferior degree of oxidation, which would turn it into mosaïc gold. It should seem, that the hydrogen, being presented to the oxigen of the oxide in a more powerful degree than in hydro-sulphuret at a maximum, saturates and converts it entirely into water, leaving none with the metal, which, as we have seen, can never form mosaïc gold without a certain portion of oxigen.

We shall now proceed to the changes effected in mosaïc gold by potash.

Mosaïc Gold and Potash.

Liquid potash, assisted by heat, quietly dissolves mosaïc gold, and assumes a greenish shade. From this solution acids separate a yellow powder, which is no longer mosaïc gold but hydro-sulphuret at the maximum: there is, therefore, a decomposition of water; the base of the mosaic gold deprives it of oxigen, to raise itself to the maximum of oxidation, whilst the hydrogen, on the other hand, combining with the sulphur, constitutes sulphurated hydrogen, and the mosaïc gold thus becomes transformed into hydro-sulphurated oxide major; or, in other words, into hydro-sulphuret of tin major: in fact, this precipitate possesses none of the properties of mosaïc gold; muriatic acid dissolves it, disengages the sulphurated hydrogen, and reduces it to a simple solution of muriate, whose basis is at a maximum.

This reminds us of that decomposition of water, which accompanies the transformation of sulphur of antimony into kermes. The antimony is oxided at the expence of the water, which it decomposes; whilst its sulphur is hydrogened, and furnishes the antimonial oxide with the requisite saturating acid. There is, however, between antimony and tin this difference: that though the latter is raised suddenly to its maximum in potash, antimony never passes its minimum in changing into kermes. Indeed, it is still more surprising to observe that sulphuret of tin, whose affinity to oxigen appears

far

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far greater than that of mosaïc gold, cannot decompose water like it.

But mosaïc gold is not singular in undergoing this inversion. For example, if oxide of tin at the minimum be heated with potash and sulphur, the oxide will be suddenly raised to the maximum, and changed into hydro-sulphuret major.

If muriate of tin minor be poured into potash, exempt from sulphurated hydrogen, it produces a yellowish precipitate, inclining to fawn, which is nothing else than hydro-sulphuret major. Oxide of tin at the minimum has, therefore, a peculiar disposition to decompose water, and to be suroxidated at its expence. Thus mosaïc gold cannot be had by the humid way. Pelletier, who went no farther than the precipitation of the muriate in sulphate of potash, thought he had obtained mosaïc gold, because his precipitate, when heated in a retort, was converted into that substance; but it did not occur to him then, that what he was heating was not, as it should have been, a composition capable of resisting acids; in a word, it was not mosaïc gold.

If all liquid sulphurets were hydrogened, as Berthollet imagines, the precipitates which they give with muriate at a minimum, would be very much mixed with black hydro-sulphuret; and, consequently, of a very deep colour; but nothing is less general.

When the precipitate is very yellow, capable of complete solution in pot-ash, and the solution does not turn brown when mixed with hydro-sulphurated water; the conclusion must be that there are simple sulphurets of potash, as well as compound ones.

But we must not forget that no liquid sulphuret is strictly without a little hydrogen, as I have demonstrated; it is this which clouds the yellow colour of the hydro-sulphuret major, and gives it a drab coloured hue; but these small portions of hydrogen cannot be considered as necessary component parts of the sulphurets; nor as mediums without which the sulphur could not be suspended in the alcali? I cannot admit this. Put diluted sulphuret of potash into three glasses, and add to two of them a little hydro-sulphuret of potash, in unequal proportions; then let a few drops of muriate of tin

tin minor fall into each, and three very different shades will immediately be perceived, which perfectly confirms all that I have here affirmed.

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Mosaïc gold, then, decomposes the water in presence of sulphuret of potash, as has been just shown; but it also decomposes it in the midst of hydro-sulphuret of potash, one of the most disoxidating compositions known. Heat mosaïc gold in hydro-sulphuret, and it will be dissolved: add to the solution an acid, the precipitate will be yellow, and exhibit all the properties of hydro-sulphuret of tin major: that is to say, sulphurated hydrogen, alone or combined, can never deprive tin of its tendency to decompose water, in order to arrive at the maximum of oxidation.

Muriate at the Maximum and Tin.

If hydrogen, assisted by the affinities which sulphur adds to those which it possesses, cannot lower the oxidation of tin, it will be conceived that hydrogen alone is still less likely to effect it; and, indeed, if thin plates of tin be heated in a solution of tin at the maximum (such as the diluted fuming muriate, or residuum of muriatic ether, an old sulphate, &c.) the oxide at a maximum separates in white flakes, which become vitreous in drying, and, in a word, possess all the properties of which we already have said so much. This is a mean of restoring the integrity of solutions which have been changed by the atmosphere. During this solution, a decomposition of water, and disengagement of hydrogen take place. This hydrogen, which under similar circumstances would lower the oxidation of iron, has not the same power over that of tin; zinc itself precipitates the oxide of tin, and the hydrogen, procured in such great abundance, has no greater effect upon this oxide.

All these Experiments prove, that the oxide of tin in passing from a minimum to a maximum decreases in solubility, and follows the same law as iron, manganese, cobalt, and many other metals; they also show why it is that acids have so little action upon the native oxides of this metal, and that potash, on the contrary, has so great an aptitude to dissolve them, as has been remarked by Morveaux, viz. that native oxide

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oxide is also at 28 in 100. In this oxide, whose fragments are equally vitreous with those of the artificial, the condensation is so great, that when heated with sulphur, it yields but slowly to disoxidation; the process must be repeated two or three times, during which it emits sulphureous gas, and is at last changed into mosaïc gold.

White crystals ought, undoubtedly, to be replaced among ores of tin, from which they have been separated for want of proper examination. It is true that tungstein has been frequently taken for the white oxide; but this last, though rare, does not less positively exist. Among a collection of minerals sent me from the mines of Monteray, in Gallicia, were three white crystals, opaque, and quite disfigured by rolling about, which I at first took to be tungstein; but finding that, after remaining in muriatic acid for a twelvemonth, they still remained unchanged, I examined them again, and found they were pure oxide; these are the same which I changed into mosaïc gold by sulphur. The grey and brown crystals also change into this substance, but with more difficulty; their mosaïc gold is contaminated with sulphuret of iron; it may be discovered by muriatic acid; it also retains some sand and small fragments of undecomposed crystals.

A phenomenon not less interesting to the sight than the judgment may be observed in the solution of indigo in potash, prepared by the medium of the oxide at the minimum. Put indigo, oxide, and liquid potash into a bottle, and stop the mouth quite close; let it be well shaken from time to time; and when the indigo has entirely disappeared and the liquor become of an orange-yellow colour, proceed to the following experiment:

Pour cold water into one glass; boiling water into a second; and hydro-sulphurated water into a third: then pour a few drops of the indigo solution into each; the water in the first glass will immediately become blue; that in the second, a beautiful orange-yellow; and that in the third, will be similar to the second. In all this we may perceive the influence of the atmospheric oxigen. The indigo being disoxidated in the solution instantly attracts the oxigen commonly suspended by cold water, and resumes its primitive colour; whilst the boiling water, being deprived of its atmospheric air, fails to produces a similar phenomenon. In the hydro-sulphurated water,

no

no change is perceived, because this water no longer contains oxigen.

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A linen or cotton rag, previously wetted with boiling water, if dipped into the second glass comes out of a yellow colour, which changes successively to green, and then to blue, with which it is finally firmly dyed. If the contents of the second glass while still yellow be thrown into a large bell-shaped vessel, and whirled round, the liquor will pass rapidly from yellow to perfect blue. The indigo having recovered its native colour, becomes insoluble and settles: in like manner, if a few drops of oxigenated muriatic acid be put into the solution whilst yellow, the blue colour will be instantly restored; but more of the acid will destroy it again. These effects confirm more and more the ideas of modern chemists respecting indigo.—In India and the islands indigo is not drawn from the vegetable juices which contain it, so much as it is precipitated by oxigen; and in Europe it is only fit for the purpose of dyeing in proportion as this oxigen is destroyed. The effects of the woad vat though so different in their appearance from those produced by the disoxidating minerals, are nevertheless subject to the same theory. The fermentation of the green fecula of the woad, or of bran, or madder, &c. disengages a portion of hydrogen, which attacks and disoxidates the indigo, and restores its green colour. After frequent opportunities of observing the process in the vat, I am persuaded that any other green plants rich in fecula, such as cabbages, and all cruciferous plants in general, would produce similar effects, and might be advantageously employed, particularly where woad cannot conveniently be obtained.

It may be interesting to commerce, and to manufacturers to be informed that 100lbs. of linen, well scoured require 6¼lbs. of indigo, to dye it of a turquoise blue, the deepest tint that can be given; this I have obtained from an experiment made some time ago, with great care, in a wood vat.

I could wish to give in this place a set of experiments made upon the scarlet, with solution of tin, by sulphuric acid, sea salt, and saltpetre, to avoid the use of aquafortis; but I wish to correct some particular parts, which I have not leisure just now to do. I can, however, assert that sulphuric acid, and even the salt, which both change scarlet to violet, appear to be

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no obstacles to the success of the scarlet dye. I have a number of patterns, of the most perfect dye: the least of these patterns is 12 inches square, which I mention lest they should be confounded with small scraps dyed in a wine glass.

Oxide of tin at the maximum is very soluble in potash: this solution easily crystallizes. The crystals appeared to me to be lenticular, adhering to each other without order: they taste like potash; dissolve in water, where they lose part of their oxide; become dense in a retort, yield water, ignite without melting, and preserve their shape. Beyond this I have not observed any thing remarkable.

Madrid, April, 1805.

V.

Experiments and Observations respecting the Manner in which the Gases are afforded in Water by Galvanism, and various essential Points of Theory. By H. B. K.

To Mr. NICHOLSON.

SIR,

Introductory observations.

SOME respectable chemists having expressed a great desire that I should give in your Journal my experiments, which form my opinion that the gases obtained in Galvanism vary from different causes, as it is an opinion that militates so directly against the Lavoiserian theory; therefore they say the experiments which lead to it, ought to be given to the public, and not rest upon a bare assertion.

I must make these general observations, that I give the name of pile to all Galvanic apparatus, even when they are formed of troughs; and also, that the same general names are given to the wires, calling them the silver wire, and the zinc wire, whatever metals formed the pile.

The Galvanic pile yields hidrogen and oxigen in water with gold wires —no gas with animal fibres— but the gas as before, if the fibres have ter-

Having constructed a pile, I found that it yielded hydrogen and oxigen gases with gold wires, and which were the common gases usually formed, and that they detonated. I then, instead of the gold wires, used either animal or vegetable substances; but I found they generated no gases. Then it would appear, that the metals are necessary to this phenomenon as

as well as the Galvanic fluid. Then instead of letting these animal and vegetable fibres enter the water that was to be acted upon, I placed two very small and short pieces of gold wire immediately in contact with the water, and then united to them the animal fibres that were connected with the plates of the pile, but there were scarce any gases produced; but I found as I lengthened these gold wires, in the same proportion, I produced gases. That being ascertained, I endeavoured to discover whether the silver or the zinc wires were more necessary to forming the gases. The silver wire was made of gold, the usual length, without any animal fibre; and the zinc wire was a very small piece of gold wire, placed in the water, united to the pile by animal fibres. Gases were equally produced, the same as if two long gold wires were used, at both the zinc and silver ends of the pile.

minations of gold wire, and more the longer the wire.

The quantities of wire and fibre at each end need not be equal.

I next endeavoured to ascertain, whether one metallic wire was better than another; and I found that they all gave gases; but the most calcinable wires in the greatest proportion. I also found, by examining the gases, that the easy calcinable wires produced the greatest proportion of inflammable gases, and platina wires the most of oxigen and azote gases. I next tried charcoal wires, and I found that they formed more gases than the animal or vegetable fibres, but less than the metals, and that the silver charcoal formed by much a greater proportion than the zinc charcoal; and, as Mr. Davy also found, that they formed neither hydrocarbonate, nor carbonic gases; but I found them gases of less inflammability than are produced from the metals, as well as less in quantity; and also a more considerable quantity of azote gas than the metallic wires produced, for there was a proportion of azote gas in the gases, whatever metallic wires were made use of; but gold and platina wires, yielded the most, but not equal to what was produced by the charcoal wires. It clearly appears, then, that metals, or charcoal, are necessary in forming the gases.

The most oxidable wires give the most gas and most inflam:—platina most of oxigen and azote.

Charcoal wires less than metals, and the silver s dem they afford most azote.

The next consideration was to find what changes these wires had undergone from producing the gases. A thin iron wire, which had for a long time been used for the silver wire, the galvanic fluid, in consequence, passing through it: upon examination it was found to have lost its ductility, and it appeared to shiver in pieces when struck with the hammer, something

The wire and charcoal were less combustible after this process.

thing like cast iron: its fibres appeared like it. It afforded less hydrogen gas, with a solution of the vitriolic acid and water, than the same weight of wire that had not been used in this galvanic process; and I should have supposed, that it would have burned with less inflammation, if it had been burned in oxigen gas: two slips of charcoal, that had been long used as galvanic wire, had lost greatly their combustibility; and when applied to the calces of metals, produced a less proportion of them, than the same quantity of charcoal would have done, that had not been galvanised. The power of the pile was by no means so strong, as to suppose there had been any combustion in any of the wires; but evidently their combustible principle or property had been extracted by the galvanic fluid, producing gases. It is clearly so, from this self evident demonstration, namely, these gases are well known to be capable of combustion. To prove this doctrine, I took incombustible bodies, as the animal fibre, well washed from all its combustible matter, for there was only the white muscular fibre left, and used it instead of the metallic, or charcoal wires, and they produced no gases, neither from the silver nor zinc sides. But when, instead of water being placed between the muscular fibres, I placed a solution of potash, I found the solution very soon nitrated. Now it must appear, that the acid which undoubtedly the galvanic fluid possesses, instead of having the inflammable substance of the wires to unite itself to, so as to form gases, united itself to the potash neutralizing it. I think these are such self evident truths, that they cannot possibly be otherwise explained.

Arguments against the Lavoisierian theory of Galvanic effects.

The Lavoisieran explanation of these phenomena, upon the composition of water, is directly contradicted by every fact. If the wires acted by aiding the galvanic fluid in decompounding the water, we cannot rationally suppose that the zinc, being the calcinable wire would produce the oxigen gas: for this theory says, that when metals are calcined by water, the metal seizes its oxigen, and its hydrogen is set free as gas. and its advocates have been pushed to this extraordinary explanation, that the galvanic fluid seizes upon the hydrogen of the water, leaving the oxigen free; but then, in this case, it would do the same when the animal fibre was used instead of the metal: but it is not so; the animal fibre forms no gas.—

Besides

Besides, we are forced upon this highly improbable opinion, that the galvanic fluid can carry this hydrogen through even the body of a man; for the phenomena take place if he forms a part of the circuit. But this circulation is directly contradicted; for it appears clear, from my experiments, that the fluid passes from the silver wire to the zinc wire; for when the animal fibre was placed at the silver side, and only connected with a small portion of metallic wire, to unite it to the water, no gas was produced; but when placed upon the zinc side, gas was produced. The zinc side only requiring a metallic body, ever so small, just to receive the fluid out of the water, if after that it had the muscular fibre united to it, it makes no difference in the production of the gases. It appears to operate by hindering the galvanic fluid which was united to the combustible part of the silver metal, from passing through it, as by that union, it had lost that tenuity or penetrability, so as to be admitted through the close pores of the metal, and the metal may also operate by its combustible matter at its point, which is in the water, uniting to the galvanic fluid and form gas. I found that when the discs were filled with ammonia, there was a greater proportion of hydrogen gas; and when they were filled with acids, more of oxigen formed at the wires: also, the larger the plates of the pile, the less of gases were produced; and I think there were more of the oxigen and azote kind in proportion; and also, the galvanic fluid had a less tearing sensation to those who were electrified by it, and evidently gave them a less shock, even when the plates were so large as to produce the combustion of metals, and when so large, they produced the most nitric acid. That tearing sensation of the pile, seems to be from the fluid being united with the combustible part of the metals, and, in consequence, circulates with less ease through the animal body; and, therefore, gives that tearing sensation. It is from this that the fluid requires water to assist it in entering and passing through the skin of animals, as the hands requiring to be moist when you touch the galvanic wire. But the pure electric fluid requires no such aid; and this is the only difference in those two fluids.—Therefore, I should think that the galvanic fluid is more adapted to remove obstructions in diseases; the one possessing so much less of combustible matter than the other.

Facts and observations in explanation of the theory of Galvanism.

That

Facts and observations in explanation of the theory of Galvanism.

That there is an acid similar to the marine in the Galvanic fluid, appears from the zinc wire, when it is silver, having some *luna cornea* upon it, even though the water that is placed between the wires is pure distilled water.

The action of the pile seems to be from two causes; the first, from two metals being united, which have different saturations of the electric fluid; therefore, by being united, the saturation of each is disturbed, producing a motion in the fluid, and the pile being formed of water in the discs, the water being not so good a conductor of the Galvanic fluid, there is an obstruction which makes an accumulation, and it accumulates to such a pitch, as to overcome the obstruction, and then, when it is overcome, there is a strong current produced like a river's obstruction, producing streams. But then the principle of the electric fluid being so repellant, the particles, when accumulated, repel each other with such power, as to give great velocity to its motion. It has this great repellant power, that it can tear buildings to pieces, when its passage is obstructed, aiding its velocity and power.

I am now describing the simple pile, with nothing but pure water and pure metals; but there is also a calcination of the zinc metal, particularly when active bodies are placed in the discs, which actively calcines the zinc metal, and by that means sets a quantity of its electric fire free. The fact that you mention, Mr. Nicholson, that metals by having the electric fluid pass through them become lighter, must add strength to my opinion, that it is the combustible part of them, which the electric fluid receives from them; for if you will chemically examine the metals after the process, they will be found to have lost this matter.

I hope I have given a regular chain of facts, which it will be found difficult to misconstrue; I shall not examine this doctrine, by an extensive examination of a variety of different facts, it being too wide a field for your journal; but just take those which appeared in your last number, and see whether they are explained by the French theory, in that simple and easy manner, as my explanations are made; doing it in a cursory way, for fear of making my paper too long. I shall first notice Mr. Northmore's additional experiments, which I think very valuable, and likely to be aiding in elucidating the truth,

truth, when exempt from the shackles of theory. Upon condensing nitrogen gas upon lime, he procured the nitrate of lime. In this experiment then, he procured the nitric acid, which he could not do in his former experiments, and there was no oxigen here to unite with the nitrogen. This directly confirms my opinion hinted at in my last, that this nitrogen gas was formed of the nitric acid, and the combustible animal matter; for Dr. Priestley only procured from the nitric acid and muscular fibre, nitrous gas, but when he exposed the ingredients to heat, he procured less nitrous gas, and when the acid was much diluted with water, and heat applied, he procured azote (nitrogen.) See his Experiments, vol. ii. beginning at p. 147. Also to shew what effect heat has upon these nitrous gases, Dr. Priestley, vol. iii. p. 328, in heating the nitrous oxide in malleable iron, says the bulk of the air was encreased, and become all phlogisticated air (nitrogen). Now when nitrogen was compressed on lime there was no oxigen to form it into the nitric acid, but the fire which the lime possessed set fire to the combustible matter it was united to, which appears from its violent explosion."

Fact and observations in explanation of the theory of Galvanism.

Experiment 9, He also compressed nitrogen gas upon the gaseous oxide of carbone, and nitrous acid was formed: now in this experiment there was no oxigen, but if he had compressed nitrogen gas upon the carbonic acid gas, he would have formed no nitric acid as I found.

His observations that nitrogen is the cause of explosions, are not just; for the strongest gunpowder is made of the ox-muriate of potash, instead of nitre, also phosphorus and sulphur explode when melted under water; besides many other examples in chemistry.

Mr. Northmore's experiments on the compressions of the oxiginated muriatic acid gas, are also very valuable, it becomes more concentrated, which adds, as he observes, to its pungency, and its volatility. But what will appear extraordinary to the advocates of the Lavoisieran theory,—when I compressed either atmospheric air or oxigen gas upon the oxigenated muriatic acid gas, they became injured, decompounding each other; the same effect as nitrous gas would have had and the oxigenated muriatic gas became the liquid muriatic acid. That compressing hydrogen upon the oxigenated muriatic

Facts and observations on explanation of the theory of Galvanism.

riatic gas instead of its becoming weaker, according to their theory, the hydrogen should have united to the oxmuriatic oxigen and formed water, but it became a stronger, more pungent and volatile gas, destroying vegetable colours more actively, just the reverse of what this French theory teaches. It appears clearly that the muriatic acid becomes a gas from inflammable matter, also that its volatility, pungency, and its power of destroying vegetable colours, proceed from this combustible matter, and that this combustible matter of this oxmuriatic gas, when united with oxigen gas, produces an active fermentation, setting loose a great quantity of free fire, the same as nitrous gas and oxigen gas do, the one mixture forming the nitrous acid, and the other the muriatic acid. This accords with the doctrine of our forefathers, that combustible matter makes bodies become volatile, and it appears extraordinary, how far we have departed from this *clear, simple, and obvious doctrine.* But I am afraid I have made the communication long enough, therefore you shall have the remainder in the next, with your permission.

I am, Sir,

Yours, &c.

H. B. K.

London, April 17, 1806.

VI.

A Chemical Examination of the Bark of the White Willow and of the Root of the Herb Bennet, compared with Quinquina; considered in a medical Point of View. By M. BOUILLON LAGRANGE,* *read before the Society of Medicine at Paris.*

Inquiry from the chemical properties of bitter vegetables, how far they may be of medical value.

IN the memoir which I read, last Floreal, before the class of Physical and Mathematical Sciences of the National Institute, on tannin and gallic acid, I mentioned, that in pursuing my researches among several vegetables, called bitter, wherein the tannin was supposed to reside, I had observed properties in some, which led me to examine them in a medical point of view. Prior to the communication of my experiments and of the reflections I had made upon them, I wished to ascertain how far the art of healing might derive

* Annales de Chimie, vol. liv. p. 287.

advantage

advantage from the two substances which are the objects of this note. I know that a slight success is not sufficient to fix the opinions of physicians; that it is useful to multiply facts, and that it remains for practitioners to decide upon so important an object. These considerations have determined me to present my labours to the society of medicine, in the persuasion that I should no where find men more enlightened, or more impartial, than are those who compose it.

Though the chemical analysis may not lead us to sure results as to the application to be made of any medicine, yet it ought at least to enlighten the physician, and give him a kind of security.

Chemical examinations can only afford probability.

Such is the object I have in view; and should my conjectures, founded on chemical principles, prove successful; medicine will not only have the advantage of rendering indigenous vegetables useful, but it will be no longer tributary to foreigners, who frequently send us the mere refuse, or articles which they would not themselves make use of.

They who devote themselves to the art of medicine healing have already some knowledge of the medicinal properties of the barks of the willow and chesnut trees, and of the root of the herb bennet. It is known that these substances are employed in some parts of Germany; and many members of this society have used them beneficially, particularly our colleagues Desessart, Coste, Willemet, &c. We want, therefore, only repeated and well attested facts. Far be from me any idea of empiricism; no one can hold it in greater detestation, or wish more strongly for its extirpation; but I believe if the society were to direct its attention towards a great number of indigenous vegetables, it would discover in some properties no less certain than those of exotics; and the facility of obtaining them would generally cause them to be preferred. The means of acquiring this knowledge are simple: let comparative essays be made; abandoning all idea of routine, which commonly confines the art to its state of infancy. It must be confessed that we are almost always deceived in the effect, expected from a medicine. The cause is sought in remote discussions; but it is near us: who does not know that for several years there has existed in commerce a great number of barks all sold under the appellation of quinquina.

Mention of some vegetables, &c.

The materia medica, but particularly quinquina, very often adulterated.

Let us, for a moment direct our attention to the apothecaries in the departments, particularly such as are at a distance from large cities: they purchase, without suspicion, these barks, persuaded that they are buying quinquina ; they make use of them ; the operation cannot answer their intention, which is attributed to the disorder. You are better acquainted than I am with the consequences resulting from the use of a bad article ; why not therefore seek for means to check the evil, and to throw light upon so pernicious an abuse ? Though we should not entirely succeed, the intention would at least be entitled to favor.

White willow bark, and root of the herb bennet.

It is long since the white willow and root of the herb bennet were ranked among vegetables proper for tanning ; they do indeed, possess qualities similar to those of oak bark ; but as much as they differ from this substance in this character, by so much do they approximate to quinquina in medicinal properties.

Of the Bark of Willow.

Examination of willow bark.

The bark of young branches appear to me to be preferable ; they should be used dry and broken. The water wherein this substance has been boiled, acquires a deep yellow tinge, bordering on red, which becomes turbid as the liquor cools.

When several decoctions have been made, the last are always the most coloured.

Its aqueous decoction.

This *decoctum* has a bitter and very rough taste. It feebly reddens tincture of turnsole ; is abundantly precipitated by the *solutum* of glue, and by the carbonates of potash and ammoniac.

Habitudes of the decoction, with various agents.

Acetate of potash and muriate of ammoniac cause but a slight precipitation ; indeed, with the muriate, it is scarcely perceptible.

If carbonate of potash be added at the time of making the decoction, the liquor assumes a deeper colour. This change seems to result from a disengagement of carbonic acid, which leaving the potash disengaged, causes it to act as an alcali upon the colouring matter of the bark, and upon a portion of the resin dissolved by the water ; for the liquor no longer becomes turbid in cooling. These phœnomena have been observed in the decoction of quinquina, by many chemists.

Lime

Lime water poured into the *decoctam* of willow bark, throws down a precipitate of a clear blue colour, which afterwards becomes fawn.

Sulphate of iron gives a dark green precipitate; if the decoction be very concentrated, it passes to black, particularly in the latter decoctions.

Many other metallic salts are also decomposed, such as nitrates of mercury, of silver, acetate of lead, sulphate o copper, and antimonial tartrite of pot-ash (tartar emetic.)

Alcohol precipitates flakes but little coloured, whilst the supernatant liquor is highly tinged.

By evaporating the *decoctum* to the consistency of syrup, and afterwards drying it on plates, an extract is obtained, dry, brilliant, separating in scales, of a beautiful red colour, rather deep, of a very bitter, acerb flavour, and possessing all the characteristics of the dry extract of quinquina, except that it attracts very little of humidity from the atmosphere. Decoction of willow bark affords a fine flaky extract like quinquina.

The alcoholic tincture of willow bark is of a yellowish green colour, very bitter to the taste, and its transparency is disturbed by water. Alcoholic tincture of willow bark.

The phœnomena produced in the *decoctum* by the addition of *solutum* of glue and sulphate of iron are also seen in the alcoholic tincture.

Lime water forms in it a blueish precipitate; which proves that the bark contains a small quantity of gallic acid soluble in alcohol.

Evaporation of the alcohol leaves a brilliant substance, of a deep yellow colour, and very bitter; it liquifies in a gentle heat, and if thrown upon hot coals, sends forth a thick aromatic smoke.

On considering all these products, we readily recognise their similarity to those obtained from quinquina. But, it may be asked, are the quantities the same? Perhaps we might answer in the affirmative; but I thought it unnecessary to make calculations of the respective quantities, as they are so very variable, even in the same species. Besides, the difficulty of making such computations in vegetable and animal compositions is well known; I even believe it to be impossible to obtain similar results in repeating experiments of this kind; and though I have not mentioned the other constituent parts of this All the experiments on willow bark are similar to those on quinquina.

this substance, which may be discovered by its complete analysis, it cannot present any uncertainty in regard to its properties. It appeared to me most essential to verify the predominant parts, those which physicians, of all times have acknowledged to possess the real and distinguished properties.

Of the Root of the Herb Bennet. (Geum urbanum, Lin.)

The roots of the herb bennet. (Geum urbanum.)

As there are several species of the herb bennet, I have mentioned the botanical name of that which should be preferred for medical purposes.

It is pretended that the word *bennet* is derived from *benedictum*, (blessed, holy); a name given to this plant by the ancients, on account of the great virtues they attributed to it.

Aqueous decoction of bennet root.

Water wherein bennet root, dried and bruised, has been boiled, acquires a deep brown colour, yielding an aromatic odour: its transparency is disturbed in cooling much more than the willow decoction. In this it resembles more nearly the decoction of quinquina. It is bitter and very acerb to the taste, and feebly reddens the tincture of turnsole.

Habitudes of the decoction with various agents.

Solution of glue causes it to throw down a very abundant precipitate; and the supernatant liquor changes to blue, on the addition of sulphate of iron.

Lime water and water of barytes, when poured into the *decoctum*, cause a flakey precipitate, of a reddish colour, bordering upon violet.

Solid caustic potash proves it contains azote; the qnantity of ammoniac disengaged from it is pretty considerable, particularly if the decoction be concentrated. The liquor then assumes a red brown colour.

Carbonates of pot-ash, of ammoniac, and acetate of potash, added to the *decoctum* of bennet root, produce very abundant precipitates.

Muriate and oxalate of ammoniac cause but slight deposits.

Sulphate of iron is precipitated of a beautiful blue colour, the supernatant liquor always preserves this tint, but not so deep; it undergoes no change on the addition of *solution* of glue.

Several other metallic solutions are also decomposed by it such as nitrates of silver and of mercury, sulphate of copper and acetate of lead.

Th

The deposit caused by antimonial tartrite of potash is so abundant, that there is reason to believe all the metallic salt is decomposed. The supernatant liquor is without colour: hydrosulphuret of pot-ash, added in whatever proportion, causes no red precipitate. Habitudes of the decoction with vorious agents.

The liquor, separated from the deposit, and filtered, no longer possesses its bitter nor acerb flavor; it reddens tincture of turnsole more deeply than the solution of tartar emetic. It still gives a precipitate with sulphate of iron, but of a green colour instead of blue; and is not affected with *solutum* of glue.

From these experiments, it may be concluded, that the extractive colouring, resinous, and tannin matter is what causes the acerb and bitter flavor, and combines with the oxide of antimony; and that the substance which remains in the liquor, and gives a green colour with sulphate of iron, is a particular acid.

M. Vauquelin attributes this effect in quinquina, rhubarb, and root of calaguala (the latter of which he has just examined), to the resin contained in these substances: but, I think, that the green colour with sulphate of iron, may be attributed to a modification of the gallic acid.

This acid, so modified, exists in a number of vegetables, which contain tannin, as is proved in my researches on this substance: it is found in the catecher, the arnica, and many other vegetables, ranked among tanning matters.

The extract obtained by evaporating the *decoctum* of bennet root, is so analogous in its characteristics, to those of quinquina, that much experience is necessary to enable us to distinguish one from the other. Extract of bennet root.

If lime be thrown into a concentrated solution of this extract, a disengagement of ammonia takes.

Alcohol also acts upon this root, and received from it a brownish tint, but not quite so deep as that which it acquires from good quinquina. Its taste is bitter and acrid, water disturbs its transparency, and it reddens the tincture of turnsole. Alcoholic tincture of bennet root.

Lime water causes a more abundant precipitation than with the alcoholic tincture of quinquina, a circumstance which proves this root to contain more tannin and gallic acid, but a little less resin than the true quinquina.

The solution of tartar emetic, is equally decomposed by this tincture. Sulphate of iron also gives from it a fine black precipitate,

cipitate, whose colour may be rendered more intense, by the addition of a few drops of oxigenated muriatic acid.

Resumption. The willow, bark, and bennet root, resemble quinquina.

The experiments above reported on willow bark and bennet root, prove the identity of these substances with quinquina of the best quality.

A simple comparison will determine our ideas on this subject.

It would be nugatory here to detail the comparative experiments made upon quinquina and the barks sold under that name; the latter are so far from possessing its characteristic and chemical properties, that too much care cannot be taken to avoid them; and I am at a loss to conceive why means have not been adopted to prevent their sale as medicinal articles.

Statement of the facts observed in quinquina.

The following are the most striking phenomena which I have observed in quinquina:

The decoction precipitates glue, is decomposed by alculine carbonates, renders the emetic solution turbid, and gives a green precipitate with sulphate of iron.

Decoctions of white willow bark, and of bennet root, present similar phenomena, except that bennet root yields a blue precipitate with sulphate of iron.

The alcoholic tincture of quinquina, differs from those of willow and bennet root, only in possessing a deeper colour.

Aqueous and dry extract of quinquina seems to me to present similar characters with willow and bennet: that of willow, however, attracts less moisture from the air.

It has a little more resin than the two drugs before examined.

It is evident, therefore, that the only difference consists in a triple more of resin, which varies according to the species of quinquina, and the method of making the extract. What is now called in commerce. good quinquina, differs but very little from these two substances, particularly bennet root.

We may conclude, that these indigenous vegetables contain, like quinquina, chiefly tannin, a colouring extractive matter, resin, and an acid, which I suppose to be a modified gallic acid in willow, quinquina, and the other substances above-mentioned, whilst it is gallic acid in the root of the herb bennet.

This comparison is not a regular analysis.

It may be observed, by the foregoing exposition, that my object has not been to enter into a regular analysis of these two

two substances, which could have rendered no service to the art of healing; but to ascertain by comparative experiments, whether the properties already attributed to willow bark and bennet root are well founded, in order to induce my colleagues to apply them in practice. Should the society think this object worthy its attention, I would recommend that, besides the appointment of a committee to examine and report, it should engage its members to employ these two substances, and to communicate to it their observations. Several physicians have already prescribed the decoction, in the manner following:

Prescription of the willow bark or bennet root.

Take root of the herb bennet, or bark of white willow, dried and bruised, one ounce, boil it in a pint and a half *(trois chopines)* of water, to the reduction of 12 ounces.

Add muriate of ammonia from half a drachm to a drachm, and syrup of orange peel, one ounce. A glass, (probably two ounces) to be taken every hour.

I do not know if either of these substances have been used either in the powder, or with opiates, or by infusion in wine, or by the alcoholic tincture mixed with wine. It would likewise be interesting to ascertain what effects would result from the external application of the decoction, or of its other preparations, in cases where quinquina is usually prescribed.

The root is perhaps preferable.

Facts and observations.

Were I permitted to deliver an opinion upon the medicinal virtues of the herb bennet, I should be disposed to ascribe the febrifuge quality to it in a greater degree than to the willow bark; for there are few substances whose chemical characters have more analogy to those of quinquina: the same observation may be made respecting the bark of the Indian chesnut-tree, (marronier d'Inde)*.

* The Society of Medicine, after hearing this Memoir read, appointed Messrs. Lafisse, Emennot, Double, Deguise, and Desfigennettes, to make comparative trials of the administration of white willow bark, root of the herb bennet, and quinquina. They were expressly commissioned to examine, with a view to verify, the febrifuge, tonic, and even antiseptic qualities, which Stoll, Cullen, Will, Gunz, Buchave, and other Danish physicians, have attributed to these substances, even in preference to quinquina, according to some of their observations.

(Note of M. Sedillot, Secretary General to the Society of Medicine.)

Explanation

VII.

Explanation of Timekeepers, constructed by the late Mr. John Arnold; *for which a Reward of £3000 was given by the Board of Longitude to his Son* Mr. J. R. Arnold. *Extracted from the Account delivered by the latter to the Commissioners.**

The measuring parts in a time-piece are the balance spring, the balance, and the escapement.

I CONSIDER the chronometrical part of the timekeeper to be confined altogether to the balance spring, the balance, and the escapement. The other parts are no more than a good horizontal movement, which may be of any dimensions from two inches and a half in diameter, to five or more, and of proportionable depth, and may be constructed to go a day, a week, a month, or even a year (though the last may not be quite so well) at the option of the maker.

Suspension of the balaece on its spring.

[About three pages next following are employed on a description of the train and construction of the box time-keeper; in which there is no singularity asserted, except that the spring of the balance, which is helical, is made to exert a power endways or edgewise, to such a degree, as very nearly to support the weight of the balance when quiescent, and actually to cause its upper pivot to press against the potence, when the re-action of the spring is near its maximum. By this means, as the writer observes, a considerable degree of friction is avoided.]

Of the Balance Spring, with the Mode of rendering it Isochronal, or of adjusting the long and short Arcs of Vibration of the Balance. The Terms long Arcs, and short Arcs, large Arcs, and small Arcs, are used indifferently.

Balance spring: helical, and of steel or gold;

The balance spring may be made of steel wire hardened and tempered, of steel wire hard rolled, or of gold wire alloyed with copper. Steel wire hardened and tempered is the

* Dated March 5, 1805. The words of the writer are retained, and the paper no otherwise shortened than by omitting what refers to the movement. *N.*

most

most elastic—then gold, and lastly, steel wire hard drawn. Springs composed of either the above substances, if the materials be good, will answer the purpose. The quantity of copper alloy put to the gold, has been found to answer in the proportion of from one eighth to a quarter, and many other proportions may probably do as well. The form of the spring is helical, or cylindrical, except for a portion of the turn at each end, where it is curved in, and fastened at an equal distance between its centre and circumference, see Plate II. Fig. 2. Were not those turns to be curved inwards, but left of the same diameter with the others, the spring would not have its present easy, concentric motion, but on the contrary, would jolt, wabble, and be distorted. Whether the balance vibrates an arc of 230 degrees from its point of rest in its forward direction, and re-vibrates 230 degrees in its backward direction, making together 460 degrees, the cylindrical figure of the spring is still preserved.

—the ends are turned in.

Upon the length of this spring depends the isochronism of the vibrations of the balance, and in every spring of a sufficient length, there is a place where all the vibrations, long, short, and intermediate, will be performed in equal times.

Isochronism depends on the length.

When the timekeeper is first set going, and always immediately after cleaning and putting into good order; the main spring pulling with all its force, the oil applied to the pivots clean and good, and every part performing its functions to the greatest advantage; the balance may vibrate from 180 to 230 degrees from the point of rest according to the power of the main spring, and the relative weight of the balance. The balance also re-vibrates on the other side of the point of rest nearly the same arc, but here we only reckon the vibration on one side.

Semi-vibrations from 180° to 230°.

From continual exertion, the main spring will undergo some diminution of its original power, and very great resistance will be created from the thickening of the oil, and from the accumulation of dirt, so that at the end of a long voyage, suppose three or four years, the arc of vibration of the balance will gradually decrease from 230 to probably 130 degrees, and so on, till in time it will come to rest. From which it must be evident that if the different arcs from 230 to 130 are not all performed in equal times, a great irregu-

They fall off,

—and ought therefore to give equal times in all arcs.

larity must from that cause take place. If the large arcs are performed in longer time than the small ones, the timekeeper will accelerate, or go faster and faster; and if the small arcs are performed in longer time than the large ones, it will retard, or go slower and slower. To adjust the long and short arcs, let the timekeeper when clean, and the balance vibrating to its greatest extent, go for a few hours, and then without stopping it, by means of the click and ratchet above the barrel cap, and a key applied to 10 the barrel arbor square, let the main spring down a turn or two, till the arc of vibration decreases from 230 to 230 degrees or thereabout.

Method of trial.

Then let it go for the same time as before, and if it goes slower with the long arcs than with the short ones, which is generally the case, shorten the spring, by drawing it through the lips of the stud *S*. Then try it again in the same manner, and so on till they are performed in equal times.

Adjustment by giving the spring its due length.

If on the contrary the short arcs should be performed in longer time than the long ones, or the long arcs be performed in less time than the short ones, which amounts to the same thing, the spring must be let out, or lengthened at the stud *S*, and so on repeatedly, until they correspond. If after letting out the spring several times, there should be no more to spare, a longer spring must be made. The length of spring in the timekeeper before us is about 18 inches.

—or by tapering.

If the spring is made of hard rolled wire, and the construction should be such as not to leave room for a spring of the usual length, and one much shorter than ordinary should be required, it will be very liable to be overstrained, if of rolled wire, and if hardened and tempered, or of gold, to break. It will however be a good deal relieved and assisted by tapering, the tapered end being pinned into the balance stud *m*.

Of the Balance, and the Mode of making it to keep the same Time in different Degrees of Temperature, or of adjusting it for the Effects of Heat and Cold.

The balance has arms of brass and steel laminæ, loaded at the end.

Fig. 1. Pl. III. The balance is screwed upon a collet fixed on the end of its axis [it has two radii or arms and] at the extremities of these arms are two shoulders *cc*, against which by two screws are fixed the expansion or compensation p[illegible]

[illegible]

ddd. These expansion pieces are composed each of two laminæ, the outside being of brass, the inside of steel. These two pieces are made out of one; the brass being melted upon the steel all in one piece. It is afterward cut into two. *d e*, the steel laminæ continued, but made round, and tapped. *gg* brass balls, or weights, alike in all respects, made to screw upon each tap. In the sides of these balls are two holes between the centre and circumference, made to receive a tool like a two pronged fork, called a fork screwdriver, to screw the balls higher or lower. *ff* two side screws to assist in making the balance of an equal weight, *hh* screws to regulate the mean time, and which are tapped into the shoulders *cc*, passing through the expansion pieces. The long and short vibrations being adjusted, I shall next shew how to make the timekeeper perform alike in heat and cold. The balance spring becomes weaker by heat, and stronger by cold, and was the balance to remain of the same diameter, it would go slower in heat, and faster in cold, supposing it to go to time when the thermometer stood at temperate. But when the spring becomes weaker by heat, the expansion pieces move *toward* the centre of the balance, carrying with them the balls *gg*, by which the diameter of the balance becomes smaller, and relatively lighter. When the balance spring becomes stronger by cold, the expansion pieces move *from* the centre of the balance; carrying with them the balls *gg*, by which the diameter of the balance becomes larger, and relatively heavier, and when after repeated trials, the balls are properly placed, at equal distances on each tap, the diameter of the balance will decrease and increase, in the same ratio as the spring decreases and increases in strength. The following is the cause of the expansion pieces moving toward the centre by heat, and from the centre by cold. As the outside lamina of the expansion piece is of brass, and expands, or lengthens more by heat than the inside lamina of steel, to which it is attached; it will be easy to conceive how the brass forces the steel inwards; and as the same lamina of brass contracts or shortens more by cold than the steel, it is obvious that it must draw it outwards. (Was the lamina of brass placed inside, and the steel outside, the balance would expand, or become larger by heat, and con-

Action of this balance in different temperatures.

Explanation.

 tract

tract or become smaller by cold, and instead of compensating the error of the spring, it would add to it.)

Adjustment of the balance by moving the pieces at the ends of the arms.

The balls *gg*, being made of equal weight, may be placed at the end of the taps at *e*, and if the timekeeper, being in a situation where the thermometer will rise to 100 degrees or more, should go faster than when placed in another situation where the thermometer will fall to 32 degrees or lower, it is a proof that the expansion pieces do too much, and that the balls are too heavy. Supposing this to be the case, screw the balls up close to the ends of the expansion pieces at *d*, and their effect will be less; because, notwithstanding the same degree of heat will occasion the expansion pieces to move inwards, the same quantity, or to describe the same angle from *c*, yet the balls will move through less space at *d* than at *e*. For it is evident, that if they could slide up to the ends of the expansion pieces, next to the arms of the balance, they would not move at all, or, at least, their motion could not be discovered by any effect that it would produce. If the timekeeper still gains in heat, reduce the balls, and screw them home again to *d*. In the next trial, should it lose in heat more than in cold, contrary to what it did before, it is a proof that the expansion pieces do not do enough, and the balls must be unscrewed toward the ends of the taps at *e*, until it keeps the same time in heat as in cold. If the balls being at the ends do not do enough, and the timekeeper still loses in heat, increase their size until the adjustment is brought within the compass of the length of the taps, where there is generally room sufficient to correct for a minute of difference in heat and cold per day. By screwing the balls up and down, it may be soon seen how much of error two or three turns will correct in a given time, and by that means discover their proper situation.

Of Positions, or the Mode of adjusting the Timekeeper to go alike, or nearly so, in different Positions.

Method of adjusting the balance so as to make equal vibrations in all positions.

The long and short vibrations being adjusted, and also the heat and cold, I shall next shew how to adjust the different positions. Let us suppose that the two mean time screws *hh*, Plate III, Fig. 1, when the balance is at rest, stand at those points

points where the hours 12 and 6 are marked upon the dial plate, and that the two side screws *ff*, stand at those points where the hours 9 and 3 are marked. If the timekeeper should go faster with the hour 12 highest (or vertical) than with the hour 6 highest, screw in the screw *h* a little at the hour 6, and unscrew the opposite screw at the hour 12, the same quantity, if it should lose most in that position, do just the contrary. The same rule is to be observed with respect to the hours 9 and 3, by the two side screws *ff*. It may however happen that the balance will not preponderate at either of these four points, or that the screws may not be sufficiently powerful to effect the purpose. In this case for the positions 12 and 6, by unscrewing a little one of the balls *gg*, and screwing in the other, we may succeed; but this method should not be practised in superior timekeepers, because by so doing, it will occasion one expansion piece to act more than it ought to do, and the other less, and destroy that equality of expansion, of weight, and of distance, which the very word *Balance* informs us ought to be preserved. To remedy this inconvenience, another method has been contrived, by which the balance may be rendered of equal weight while the balls, the screws, and every opposite part, are at equal distances from the centre. Let the balance be made with a light ring *xxx* (as in Fig. 1, Pl. II.) within the expansion pieces. Let there be three light equal weights *kkk*, which by a screw in each may be fixed upon any part of the ring; then having adjusted the long and short vibrations, and the heat and cold, and having the mean time screws at equal distances from the centre, and the balls at equal distances upon the taps (there will be no occasion for side screws), try the timekeeper in different positions, and in a very few trials, by moving the weights upon different parts of the ring the positions may be adjusted very accurately. The weights may be brought all to the same part, and the balance made to preponderate in any given point, and none of the other adjustments will be affected by it, and the weights, upon whatever parts of the circle they may be, will still remain at an equal distance from the centre. Having adjusted for long and short vibrations, heat and cold, and positions, it remains only to regulate for mean time. Should the timekeeper gain, increase the diameter of the balance by drawing out

The rule here given seems to be,—*shift the centre of gravity towards that part which when highest gives the quickest rate.*

out an equal quantity of the two mean time screws *hh*, and should it lose, decrease the diameter, by screwing in an equal quantity of the same screws. This adjustment does not affect for that heat and cold, because these screws are unconnected with the expansion pieces, nor will they affect the positions, if they are both turned the same quantity, and the taps of the same thread.

Of the Escapement.

Description and effects of the detached escapement as constructed by Arnold.

Fig. 5, represents the escapement wheel, [supposed to be urged to motion by the train] the teeth of which are of a cycloidal shape, and whose upper surface towards the extremity presents to the view a triangular form, two sides of which are described by right lines, and the other by a cycloidal curve, which is the principal part of action.

In this plan the whole of the escapement wheel is thrown open to view; *BBd* the escapement or locking spring, screwed fast by its end *C* to the pillar *D*, and extending from *C* to *d* in the direction *CBNBd*. The centre of motion of this spring is between *C* and *N*, the part *NBd* being more substantial than the part *CBN*, and into which part between *N* and *B* is fixed the locking piece *a*, the locking piece or locking pallat, whose acting surface is a jewel (see also Fig. 4,) placed between *N* and *B* and opposite the end of an adjusting screw *F*, which pallat descending from the escapement spring, locks upon the interior angle of the tooth 2, and upon every tooth in succession, suspending the motion of the escapement wheel for a time, and leaving the balance to vibrate without interruption from any part of the machinery. It is to be observed, that the triangular parts of the teeth of the wheel *AAA*, the wheel being hollowed or sunk, are raised above the periphery of the wheel to meet the locking piece *a*, so that upon viewing the wheel edgewise, see Fig. 5, the teeth will appear broader than the edge of the periphery *b*. In Fig. 3, the locking pallat *a* being in contact with the tooth 2, is not so well distinguished as in Figures 6, 7, and 8, where it appears very plainly over the periphery *b*, of the wheel in the interval between the teeth 1 and 2.

Fig. 4, gives a view of the escapement spring reversed, and Fig. 9 explains upon a large scale the figure of the locking piece, which is angular, adjoining that part of the straight edge

edge where the locking is effected. Was this angular part to be left square, like the opposite end, it might strike against the interior angle of the tooth, as the escapement spring returns to its place against the adjusting screw, after having unlocked or discharged the wheel, but by being of this figure it clears itself.

Description and effects of the detached escapement as constructed by Arnold.

Ne the discharging or unlocking spring, which is attached to the escapement or locking spring at *N*, and passes under the adjusting screw *F*, a little beyond the end *d* of the locking spring. This discharging spring is made very slight and delicate.

F the adjusting screw supported by the pillar *g*, whose end is opposite to the locking piece or pallat *a*, on the contrary side of the escapement or locking spring, and by which the locking piece or pallat *a*, may be more or less advanced upon any tooth of the wheel; the escapement spring *BBd* always pressing the locking pallat *a*, against the end of the screw *F*, except at the time of unlocking the wheel.

o, The unlocking, discharging or small pallat, whose part of action is a jewel. This pallat when the balance is in motion, presses against the end of the discharging spring at *e*, and passing on in a direction from *e* to *d*, carries with it for a short space, the discharging spring *Ne*, and also the locking spring *BBd*, moving them both at the same time, and in so doing carries the locking piece *a*, from off the interior angle of the tooth 2, (or any other tooth which may come into that situation) and leaves the wheel at liberty to impart its power to the impelling pallat. But when the balance returns, and the unlocking pallat *o* repasses the discharging spring *Ne* in a direction from *d* to *e*, it does not in the least disturb the locking spring *BBd*, nor consequently the locking piece or pallat *a*, but moves only, and for a short space, the unlocking spring *Ne*. *HHH* The impelling or large pallat, whose part of action is at the angle *m*, where a jewel is placed. Upon the exterior of this angle the pallat receives its impulse from the cycloidal part of the tooth of the escapement wheel. The circumference of the pallat is incomplete from a portion being cut away to make room for the action of the teeth of the wheel.

X. is a circular hole under the periphery *b*, of the escapement wheel, over the centre of which the tooth 2 appears, and

Description and effects of the detatched escapement as constructed by Arnold.

and the locking piece or pallat *a*. The two springs *BBd* and *Ne* pass over it. This hole is made through the brass plate *QQQQ*, through which by inverting the escapement the manner in which the locking piece *a* holds the tooth 2 (or any other tooth in succession) of the escapement wheel may be seen.

The impelling pallat *HHH* is supposed to be vibrating freely from *r* to *S*; here it is perfectly detatched, or at liberty from the escapement wheel; as will be seen by observing that the unlocking pallat *o* is not in contact with the discharging spring *Ne*, nor are either of the teeth 3 or 4 of the wheel in contact with the impelling pallat. The balance or the impelling pallat (for they are both upon the same axis) vibrating from *r* to *S*, the discharging pallat *o* comes in contact with the discharging spring *Ne*, see Fig. 6, (and from that instant it is not detached or free, but in the act of escaping) and moving it in the direction from *e* to *d*, takes the locking pallat *a* from off the interior angle of the tooth 2, and sets the escapement wheel at liberty for the tooth 3 to act upon the angle *m* of the impelling pallat. Here the tooth 2 will be seen to have passed the locking piece *a*, and the tooth 1 to approach it. The tooth 3 pressing the impelling pallat from *r* to *S*, and continuing to do so as in Fig. 7, where the centre of the escapement wheel, the angle *m*, of the impelling pallat, and the centre *G* of both pallats form a straight line. This action of the wheel upon the pallat continuing as in Fig. 8, where the point of the tooth 3 is about to quit the angle *m* of the impelling pallat, the tooth 2 approaching nearer to the circumference of the same pallat, and the tooth 1 advancing toward the locking piece or pallat *a*, against which it falls, and is held fast, as soon as the end of the tooth 3 quits the impelling pallat. Here the act of escaping ends, and the impelling pallat is again detatched or unconnected with the wheel, and moves in free vibration as in Fig. 3, for a certain number of degrees, until it is returned by the power of the balance spring and repasses from *S* to *r*, still independent of the escapement wheel or of any thing else, except the very little resistance which is encountered by the discharging pallat *o*, in repassing the discharging spring *Ne*, which it does without disturbing the locking piece *a*, or consequently the escapement wheel, and con-

continuing for a certain number of degrees, is again returned by the balance spring from r to S, when resuming its situation, it is prepared to act as before.

Description and effects of the detatched escapement as constructed by Arnold.

It may be proper to remark that the action of the cycloidal tooth upon the impelling pallat is always the same, in the beginning as in Fig. 6, in the middle as in Fig. 7, and at the end as in Fig. 8, impelling the pallat with the same quantity of surface in action at all times, and at all times equidistant from the axis of the pallat. This however depends upon the tooth having the true figure.

The proper shape of the cycloid is found in the following manner. Having a plate of smooth metal, fix upon it a piece of brass the size of the intended escapement wheel, which call the false wheel. Then take another piece of brass the size of the intended pallat, which call the false pallat. On the circumference of the false pallat fix a fine steel point, and then rolling the false pallat upon the circumference of the false wheel, the steel point will describe a line on the plate, which will be the proper curve, in which shape the tooth must be cut by an engine. The larger the pallat in proportion to the escapement wheel, the less sudden the cycloidal curve will be, and the smaller the pallat the more sudden; so that an escapement wheel which has 15 teeth, with a pallat of a proportionable diameter, will have its teeth of a very different shape to those in a wheel which has only 12 teeth, because, in one case, the pallat is half the size, and in the other it is little more than one third.

The size of the pallat depends upon the number of teeth in the escapement wheel. The radius of the pallat should be equal to the distance between any two teeth of the wheel, and then their relative motion will be equal. If the wheel has twelve teeth, the radius of the pallat will be thirty degrees, measured on the diameter of the wheel, and its diameter sixty degrees, measured in the same manner, which will make it half the size of the wheel. If it has thirteen teeth the pallat will in diameter measure fifty-five degrees and a half. If fourteen teeth, fifty-one degrees and a half, and if fifteen teeth, which is the number generally applied to pocket time-keepers, it will be forty-eight degrees.

Quickness of the train of the timekeepers

The Marine Timekeeper, which has been here described, is

Description and effects of the detatched escapement as constructed by Arnold.

is made to beat half seconds, the balance making 240 vibrations both ways in a minute. For if the balance wheel has 15 teeth, the fourth wheel 80 teeth, and the balance pinion 10 teeth, there will be 120 beats, or half seconds, in one minute.

It is also made with the escapement wheel of 12 teeth, the balance pinion having 7, and the fourth wheel 70; consequently there will be 120 beats, or half seconds, in one minute as before. It has been already remarked that the pallat for 12 teeth must be half the diameter of the wheel, and for 15 teeth five-twelfths, or fifty degrees.

The pocket timekeepers, that they may not be disturbed by motion, have what is called a quicker train, the seconds hand making 150 beats upon the dial, or 5 beats in two seconds. The escapement wheel has 15 teeth, the balance pinion 8 teeth, and the fourth wheel 80, consequently there will be 150 beats in one minute. The pallat being 50 degrees in diameter, measured upon the diameter of the balance wheel.

No mention has been made of the numbers of the teeth in the other wheels and pinions, as they are of little or no importance, and may be varied considerably.

VIII.

An Essay on the Cohesion of Fluids. By THOMAS YOUNG, *M. D. For. Sec. R. S.**

I. *General Principles.*

General principles of the cohesion of fluids.

It has already been asserted, by Mr. Monge and others, that the phenomena of capillary tubes are referable to the cohesive attraction of the superficial particles only of the fluids employed, and that the surfaces must consequently be formed into curves of the nature of lintearia, which are supposed to be the results of a uniform tension of a surface, resisting the pressure of a fluid, either uniform, or varying according to a given law. Segner, who appears to have been the first that maintained

* Philos. Trans. 1805.

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a similar opinion, has shown in what manner the principle may be deduced from the doctrine of attraction, but his demonstration is complicated, and not perfectly satisfactory; and in applying the law to the forms of drops, he has neglected to consider the very material effects of the double curvature, which is evidently the cause of the want of a perfect coincidence of his experiments with his theory. Since the time of Segner, little has been done in investigating accurarely and in detail the various consequences of the principle.

General principles of the cohesion of fluids.

It will perhaps be most agreeable to the experimental philosopher, although less consistent with the strict course of logical argument, to proceed in the first place to the comparison of this theory with the phenomena, and to inquire afterwards for its foundation in the ultimate properties of matter. But it is necessary to premise one observation, which appears to be new, and which is equally consistent with theory and with experiment; that is, that for each combination of a solid and a fluid, there is an appropriate angle of contact between the surfaces of the fluid, exposed to the air, and to the solid. This angle, for glass and water, and in all cases where a solid is perfectly wetted by a fluid, is evanescent: for glass and mercury, it is about 140° in common temperatures, and when the mercury is moderately clean.

II. *Form of the Surface of a Fluid.*

Form of the surface of a fluid, as modified by the cohesion of its parts, &c.

It is well known, and it results immediately from the composition of forces, that where a line is equally distended, the force that it exerts, in a direction perpendicular to its own, is directly at its curvature; and the same is true of a surface of simple curvature; but where the curvature is double, each curvature has its appropriate effect, and the joint force must be as the sum of the curvatures in any two perpendicular directions. For this sum is equal, whatever pair of perpendicular directions may be employed, as is easily shown by calculating the versed sines of two equal arcs taken at right angles in the surface. Now when the surface of a fluid is convex externally, its tention is produced by the pressure of the particles of the fluid within it, arising from their own weight, or from that of the surrounding fluid; but when the surface is concave, the tension is employed in counteracting the pressure of

Form of the surface of a fluid, as modified by the cohesion of its parts, &c.

the atmosphere, or, where the atmosphere is excluded, the equivalent pressure arising from the weight of the particles suspended from it by means of their cohesion, in the same manner as, when water is supported by the atmospheric pressure in an inverted vessel, the outside of the vessel sustains a hydrostatic pressure proportionate to the height; and this pressure must remain unaltered, when the water, having been sufficiently boiled, is made to retain its situation for a certain time by its cohesion only, in an exhausted receiver. When, therefore, the surface of the fluid is terminated by two right lines, and has only a simple curvature, the curvature must be every where as the ordinate; and where it has a double curvature, the sum of the curvatures in the different directions must be as the ordinate. In the first case, the curve may be constructed by approximation, if we divide the height at which it is either horizontal or vertical into a number of small portions, and taking the radius of each portion proportional to the reciprocal of the height of its middle point or below the general surface of the fluid, go on to add portions of circles joining each other, until they have completed as much of the curve as is required. In the second case, it is only necessary to consider the curve derived from a circular basis, which is a solid of revolution; and the centre of that circle of curvature, which is perpendicular to the section formed by a plane passing through the axis, is in the axis itself, consequently in the point where the normal of the curve intersects the axis: we must therefore here make the sum of this curvature, and that of the generating curve, always proportional to the ordinate. This may be done mechanically, by beginning at the vertex, where the two curvatures are equal, then, for each succeeding portion, finding the radius of curvature by deducting the proper reciprocal of the normal, at the beginning of the portion, from the ordinate, and taking the reciprocal of the remainder. In this case the analysis leads to fluxional equations of the second order, which appear to afford no solution by means hitherto discovered; but the cases of simple curvature may be more easily subjected to calculation.

III. *Analysis of the simplest Forms.*

On the simplest forms of the surface of a fluid.

Supposing the curve to be described with an equable angu-

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lar velocity, its fluxion, being directly as the radius of curvature, will be inversely as the ordinate, and the rectangle contained by the ordinate and the fluxion of the curve will be a constant quantity; but this rectangle is to the fluxion of the area, as the radius to the cosine of the angle formed by the curve with its fluxion; and the fluxion of the area varying as the cosine, the *area* itself will vary as the sine of this angle, and *will be equal to the rectangle contained by the initial ordinate, and the sine corresponding to each point of the curve in the initial circle of curvature.* Hence it follows first, that *the whole area included by the ordinates where the curve is vertical and where it is horizontal, is equal to the rectangle contained by the ordinate and the radius of curvature;* and, secondly, that the area on the convex side of the curve, between the vertical tangent and the least ordinate, is equal to the whole area on the concave side of the curve between the same tangent and the greatest ordinate.

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In order to find the ordinate corresponding to a given angular direction, we must consider that the fluxion of the ordinate at the vertical part, is equal to the fluxion of the circle of curvature there, that, in other places, it varies as the radius of curvature and the sine of the angle formed with the horizon conjointly, or as the ordinate inversely, and directly as the sine of elevation; therefore the fluxion of the ordinate multiplied by the ordinate is equal to the fluxion of any circle of curvature multiplied by its corresponding height, and by the sine, and divided by the radius: but the fluxion of the circle multiplied by the sine and divided by the tadius is equal to the fluxion of the versed sine; therefore the ordinate multiplied by its fluxion is equal to the initial height multiplied by the fluxion of the versed sine in the corresponding circle of curvature; *and the square of the ordinate is equal to the rectangle contained by the initial height and twice the versed sine, increased by a constant quantity.* Now at the highest point of the curve, the versed sine becomes equal to the diameter, and the square of the initial height to the rectangle contained by the initial height and twice the diameter, with the constant quantity: the constant quantity is therefore equal to the rectangle contained by the initial height and its difference from twice he diameter: *this constant quantity is the square*

of

On the simplest forms of the surface of a fluid.

of the least ordinate, and *the ordinate is every where a mean proportional between the greatest height and the same height diminished by twice the versed sine of the angular depression in the corresponding circle of curvature.* Again, *at the vertical point, the square of the ordinate is equal to the square of the greatest height diminished by the rectangle contained by this height and the diameter of the correcting circle of curvature*, a rectangle which is constant for every fluid, and which may be called *the appropriate rectangle: deducting this rectangle from the square of the ordinate at the vertical point, we have the least ordinate;* which consequently vanishes *when the square of the ordinate at the vertical point is equal to the appropriate rectangle; the horizontal surface becoming in this case an asymptote to the curve, and the square of the greatest ordinate being equal to twice the appropriate rectangle, and the greatest ordinate to twice the diameter of the corresponding circle of curvature:* so that, *if we suppose a circle to be described, having this ordinate for a diameter, the chord of the angular elevation in this circle will be always equal to the ordinate at each point,* and *the ordinate will vary as the sine of half the angle of elevation whenever the curve has an asymptote.* Mr. Fuss has demonstrated, in the third volume of the *Acta Petropolitana*, some properties of the arch of equilibrium under the pressure of a fluid, which is the same as one species of the curves here considered. The series given by Euler in the second part of the same volume, for the elastic curve, may also be applied to these curves.

IV. *Application to the Elevation of particular Fluids.*

Application of the doctrine to particular fluids.

The simplest phenomena, which afford us data for determining the fundamental properties of the superficial cohesion of fluids, are their elevation and depression between plates and in capillary tubes, and their adhesion to the surfaces of solids which are raised in a horizontal situation to a certain height above the general surface of the fluids. When the distance of a pair of plates, or the diameter of a tube, is very minute, the curvature may be considered as uniform, and the appropriate rectangle may readily be deduced from the elevation, recollecting that the curvature in a capillary tube is double, and the height therefore twice as great as between two plates.

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In the case of the elevation of a fluid in contact with a horizontal surface, the ordinate may be determined from the weight required to produce a separation; and the appropriate rectangle may be found in this manner also, the angle of contact being properly considered, in this as well as in the former case. It will appear that these experiments by no means exhibit an immediate measure of the mutual attraction of the solid and fluid, as some authors have supposed.

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Sir Isaac Newton asserts, in his Queries, that water ascends between two plates of glass at the distance of one hundredth of an inch, to the height of about one inch; the product of the distance and the height being about .01; but this appears to be much too little. In the best experiment of Mussehenbroek, with a tube, half of the product was .0195; in several of Weitbrecht, apparently very accurate, 0214. In Monge's experiments on plates, the product was 2.6 or 2.7 lines, about .0210. Mr. Atwood says that for tubes, the product is .0530, half of which is .0265. Untill more accurate experiments shall have been made, we may be contented to assume .02 for the rectangle appropriate to water, and .04 for the product of the height in a tube by its bore. Hence, when the curve becomes infinite, its greatest ordinate is .2, and the height of the vertical portion, or the height of ascent against a single vertical plane .14, or nearly one-seventh ofan inch.

Now when a horizontal surface is raised from a vessel of water, the surface of the water is formed into a lintearia to which the solid is a tangent at its highest point, and if the solid be still further raised, the water will separate: the surface of the water, being horizontal at the point of contact, cannot add to the weight tending to depress the solid, which is therefore simply the hydrostatic pressure of a column of water equal in height to the elevation, in this case one-fifth of an inch, and standing on the given surface. The weight of such a column will be 50 grains and a half for each square inch; and in Taylor's well known experiment the weight required was 50 grains. But when the solid employed is small, the curvature of the horizontal section of the water, which is convex externally, will tend to counteract the vertical curvature, and to diminish the height of separation; thus if a disc of an inch in diameter were employed, the curvature in this direction would

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would perhaps be equivalent to the pressure of about one-hundredth of an inch, and might reduce the height from .2 to about 19, and the weight in the same proportion. There is, however, as great a diversity in the results of different experiments on the force required to elevate a solid from the surface of a fluid, as in those of the experiments in capillary tubes: and, indeed, the the sources of error appear to be here more numerous. Mr. Achard found that a disc of glass, one inch and a half French in diameter, required, at 69° of Fahrenheit, a weight of 91 French grains to raise it from the surface of the water; this is only 37 English grains for each square inch; at $44\frac{1}{2}$° the force was $\frac{1}{14}$ greater, or 39 grains and a half; the difference being $\frac{1}{143}$ for each degree of Fahrenheit. It might be inferred from these experiments, that the height of ascent in a tube of a given bore, which varies in the duplicate ratio of the height of adhesion, is diminished about $\frac{1}{180}$ for every degree of Fahrenheit that the temperature is raised above 50°; there was, however, probably some considerable source of error in Achard's experiments, for I find that this diminution does not exceed $\frac{1}{1000}$. The experiments of Mr. Dutor make the quantity of water raised equal to 44.1 grains for each square inch. Mr. Achard found the force of adhesion of fulfuric acid to glass, at 69° of Fahrenheit, 1.26, that of water being 1, hence the height was as 69 to 1, and its square as .47 to 1, which is the corresponding proportion for the ascent of the acid in a capillary tube, and which does not very materially differ from the proportion of 395 to 1, assigned by Barruel for this ascent, Musschenbroek found it .8 to 1, but his acid was probably weak. For alcohol the adhesion was as .593, the height as .715, and its square as .510; the observed proportion in a tube, according to an experiment of Musschenbroek, was about .550, according to Carré from .400 to .440. The experiments on sulfuric ether do not agree quite so well, but its quality is liable to very considerable variations. Dutour found the adhesion of alcohol .58, that of water being 1.

With respect to mercury, it has been shown by Professor Casbois of Metz, and by others, that its depression in tubes of glass depends on the imperfection of the contact, and that when it has been boiled in the tube often enough to expel all foreign

foreign particles, the surface may even become concave instead of convex, and the depression be converted into an elevation. But in barometers, constructed according to the usual methods, the angle of the mercury will be found to differ little from 140°; and in other experiments, when proper precautions are taken, the inclination will be nearly the same. The determination of this angle is necessary for finding the appropriate rectangle for the curvature of the surface of mercury, together with the observations of the quantity of depression in tubes of a given diameter. The table published by Mr. Cavendish from the experiments of his father, Lord Charles Cavendish, appears to be best suited for this purpose. I have constructed a diagram, according to the principles already laid down, for each case, and I find that the rectangle which agrees best with the phenomena is .01. The mean depression is always .015, divided by the diameter of the tube: and in tubes less than half an inch in diameter, the curve is very nearly elliptic, and the central depression in the tube of a barometer may be found by deducting from the corresponding mean depression the square root of one-thousandth part of its diameter. There is reason to suspect a slight inaccuracy towards the middle of Lord Charles Cavendish's Table, from a comparison with the calculated mean depression, as well as from the results of the mechanical construction. The ellipsis approaching nearest to the curve may be determined by the solution of a biquadratic equation.

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Diameter in inches.	Grains in an inch. C.	Mean depression by calculation. Y.	Central depression by observation. C.	Central depression by formula. Y.	Central depression by diagram. Y.	Marginal depression by diagram. Y.
.6	972	.025	.005	(.001)	.005	.066
.5	675	.030	.007	.008	.007	.067
.4	432	.037	.015	.017	.012	.069
.35	331	.043	.025	.024	.017	.072
.30	243	.050	.036	.033	.027	.079
.25	169	.060	.050	.044	.038	.086
.20	108	.075	.067	.061	.056	.096
.15	61	.100	.092	.088	.085	.116
.10	27	.150	.140	.140	.140	.161

The square root of the rectangle .01, or .1, is the ordinate where the curve would become vertical if it were continued; but in order to find the height at which it adheres to a vertical surface,

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surface, we must diminish this ordinate in the proportion of the sine of 25° to the sine of 45°, and it will become .06, for the actual depression in this case. The elevation of the mercury that adheres to the lower horizontal surface of a piece of glass, and the thickness at which a quantity of mercury will stand when spread out on glass, supposing the angle of contact still 140°, are found, by taking the proportion of the sines of 20° and of 70° to the sine of 45°, and are therefore .0484 and .1330 respectively. If, instead of glass, we employed any surface capable of being wetted by mercury, the height of elevation would be .141, and this is the limit of thickness of a wide surface of mercury supported by a substance wholly incapable of attracting it. Now the hydrostatic pressure of column of mercury .0484 in thickness on a disc of one inch diameter would be 131 grains; to this the surrounding elevation of the fluid will add about 11 grains for each inch of the circumference, with some deduction for the effect of the contrary curvature of the horizontal section, tending to diminish the height; and the apparent cohesion thus exhibited will be about 160 grains, which is a little more than four times as great as the apparent cohesion of glass and water. With a disc 11 lines in diameter Mr. Dutour found it 194 French grains, which is equivalent to 152 English grains, instead of 160, for an inch, a result which is sufficient to confirm the principles of the calculation. The depth of a quantity of mercury standing on glass I have found by actual observation, to agree precisely with this calculation. Segner says that the depth was .1358, both on glass and on paper: the difference is very trifling, but this measure is somewhat too great for glass, and too small for paper, since it appears from Dutour's experiments, that the atration of paper to mercury is extremely weak.

If a disc of a substance capable of being wetted by mercury, an inch in diameter, were raised from its surface in a position perfectly horizontal, the apparent cohesion should be 381 grains, taking .141 as the height: and for a French circular inch, 433 grains, or 528 French grains. Now, in the experiments of Morveau, the cohesion of a circular inch of gold to the surface of mercury appeared to be 446 grains, of silver 429, of tin 418, of lead 397, of bismuth 372, of zinc 204, of copper 142, of metallic antimony 126, of iron 115, of cobalt 8: and this order is the same with that in which the metals

tals are most easily amalgamated with mercury. It is probable that such an amalgamation actually took place in some of the experiments, and affected their results, for the process of amalgamation may often be observed to begin almost at the instant of contact of silver with mercury; and the want of perfect horizontality appears in slight degree to have affected them all. A deviation of one-fiftieth of an inch would be sufficient to have produced the difference between 446 grains and 528; and it is not impossible that all the differences, as far down as bismuth, may have been accidental. But if we suppose the gold only to have been perfectly wetted by the mercury, and all the other numbers to be in due proportions, we may find the appropriate angle for each substance by deducting from 180°, twice the angle of which the sine is to the radius as the apparent cohesion of each to 446 grains; that is, for gold .1, for silver about 97, for tin .95, for lead .90, for bismuth .85 for zinc .46, for copper .32, for antimony .29, for iron 26, and for cobalt .02, neglecting the surrounding elevation, which has less effect in proportion as the surface employed is larger. Gellert found the depression of melted lead in a tube of glass multiplied by the bore equal to about .0054.

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It would perhaps be possible to pursue these principles so far as to determine in many cases the circumstances under which a drop of any fluid would detach itself from a given surface. But it is sufficient to infer, from the law of the superficial cohesion of fluids, that the linear dimensions of similar drops depending from a horizontal surface must vary precisely in the same ratio as the heights of ascent of the respective fluids against a vertical surface, or as the square root of the heights of ascent in a given tube; hence the magnitudes of similar drops of different fluids must vary as the cubes of the square roots of the heights of ascent in a tube. I have measured the heights of ascent of water and of diluted spirit of wine in the same tube, and I found them nearly as 100 to 64: a drop of water falling from a large sphere of glass weighed 1.8 grains, a drop of the spirit of wine about .85, instead of .82, which is nearly the weight that would be inferred from the consideration of the heights of ascent, combined with that of the specific gravities. We may form a conjecture respecting the probable magnitude of a drop by

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inquiring what must be the circumference of the fluid, that would support by its cohesion the weight of a hemisphere depending from it: this must be the same as that of a tube, in which the fluid would rise to the height of one-third of its diameter; and the square of the diameter must be three times as great as the appropriate product; or, for water .12; whence the diameter would be .35, or a little more than one-third of an inch, and the weight of the hemisphere would be 2.8 grains. If more water were added internally, the cohesion would be overcome, and the drop would no longer be suspended, but it is not easy to calculate what precise quantity of water would be separated with it. The form of a bubble of air rising in water is determined by the cohesion of the internal surface of the water exactly in the same manner as the form of a drop of water in the air. The delay of a bubble of air at the bottom of a vessel appears to be occasioned by a deficiency of the pressure of the water between the air and the vessel; it is nearly analogous to the experiment of making a piece of wood remain immersed in water, when perfectly in contact with the bottom of the vessel containing it. This experiment succeeds however far more readily with mercury, since the capillary cohesion of the mercury prevents its insinuating itself under the wood.

V. *Of apparent Attractions and Repulsions.*

On the apparent attractions and repulsions of floating bodies.

The apparent attraction of two floating bodies, round both of which the fluid is raised by cohesive attraction, is produced by the excess of the atmospheric pressure on the remote sides of the solids above its pressure on their neighbouring sides: or, if the experiments are performed in a vacuum, by the equivalent hydrostatic pressure or suction derived from the weight and immediate cohesion of the intervening fluid. This force varies ultimately in the inverse ratio of the square of the distance; for, if two plates approach each other, the height of the fluid that rises between them is increased in the simple inverse ratio of the distance; and the mean action, or negative pressure, of the fluid on each particle of the surface is also increased in the same ratio. When the floating bodies are both surrounded by a depression, the same law prevails, and its demonstration is still more simple and obvious. The repul-

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repulsion of a wet and a dry body does not appear to follow the same proportion: for it by no means approaches to infinity upon the supposition of perfect contact; its maximum is measured by half the sum of the elevation and depression on the remote sides of the substances, and as the distance increases, this maximum is only diminished by a quantity, which is initially as the square of the distance. The figures of the solids concerned modify also sometimes the law of attraction, so that, for bodies surrounded by a depression, there is sometimes a maximum, beyond which the force again diminishes: and it is hence that a light body floating on mercury, in a vessel little larger than itself, is held in a stable equilibrium without touching the sides. The reason of this will become apparent, when we examine the direction of the surface necessarily assumed by the mercury in order to preserve the appropriate angle of contact, the tension acting with less force when the surface attaches itself to the angular termination of the float in a direction less horizontal.

The apparent attraction produced between solids by the interposition of a fluid does not depend on their being partially immersed in it; on the contrary, its effects are still more powerfully exhibited in other situations; and, when the cohesion between two solids is increased and extended by the intervention of a drop of water or of oil, the superficial cohesion of these fluids is fully sufficient to explain the additional effect. When wholly immersed in water, the cohesion between two pieces of glass is little or not at all greater than when dry; but if a small portion only of a fluid be interposed, the curved surface, that it exposes to the air, will evidently be capable of resisting as great a force as it would support from the pressure of the column of fluid that it is capable of sustaining in a vertical situation; and in order to apply this force, we must employ in the separation of the plates, as great a force as is equivalent to the pressure of a column appropriate to their distance. Morveau found that two discs of glass, 3 inches French in diameter, at the distance of one-tenth of a line, appeared to cohere with a force of 4719 grains, which is equivalent to the pressure of a column 23 lines in height: hence the product of the height and the distance of the plates is 2.3 lines instead of 2.65, which was the result of Monge's experiments on

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on the actual ascent of water. The difference is much smaller than the difference of the various experiments on the ascent of fluids; and it may easily have arisen from a want of perfect parallelism in the plates; for there is no force tending to preserve this parallelism. The error, in the extreme case of the plates coming into contact at one point, may reduce the apparent cohesion to one half.

The same theory is sufficient to explain the law of the force by which a drop is attracted towards the junction of two plates inclined to each other, and which is found to vary in the inverse ratio of the square of the distance; whence it was inferred by Newton that the primitive force of cohesion varies in the simple inverse ratio of the distance, while other experiments lead us to suppose that cohesive forces in general vary in the direct ratio of the distance. But the difficulty is removed by considering the state of the marginal surface of the drop. If the plates were parallel, the capillary action would be equal on both sides of the drop: but when they are inclined, the curvature of the surface at the thinnest part requires a force proportionate to the appropriate height to counteract it; and this force is greater than that which acts on the opposite side. But if the two plates are inclined to the horizon, the deficiency may be made up by the hydrostatic weight of the drop itself; and the same inclination will serve for a larger or a smaller drop at the same place. Now when the drop approaches to the line of contact, the difference of the appropriate heights for a small drop of a given diameter will increase as the square of the distance decreases; for the fluxion of the reciprocal of any quantity varies inversely as the square of that quantity: and, in order to preserve the equilibrium, the sine of the angle of elevation of the two plates must be nearly in the inverse ratio of the square of the distance of the drop from the line of contact, as it actually appears to have been in Hauksbee's experiments.

VI. *Physical Foundation of the Law of superficial Cohesion.*

Law of superficial cohesion.

We have now examined the principal phenomena which are reducible to the simple theory of the action of the superficial particles of a fluid. We are next to investigate the natural foundations upon which that theory appears ultimately at rest. We

We may suppose the particles of liquids, and probably those of solids also to possess that power of repulsion which has been demonstratively shown by Newton to exist in aeriform fluids, and which varies in the simple inverse ratio of the distance of the particles from each other. In airs and vapours this force appears to act uncontrolled; but in liquids, it is overcome by cohesive force, while the particles still retain a power of moving freely in all directions; and in solids the same cohesion is accompanied by a stronger or weaker resistance to all lateral motion, which is perfectly independent of the cohesive force, and which must be cautiously distinguished from it. It is simplest to suppose the force of cohesion nearly or perfectly constant in its magnitude, throughout the minute distance to which it extends, and owing its apparent diversity to the contrary action of the repulsive force, which varies with the distance. Now in the internal parts of a liquid these forces hold each other in a perfect equilibrium, the particles being brought so near that the repulsion becomes precisely equal to the cohesive force that urges them together: but whenever there is a curved or angular surface, it may be found by collecting the actions of the different particles, that the cohesion must necessarily prevail over the repulsion, and must urge the superficial parts inwards with a force proportionate to the curvature, and thus produce the effect of a uniform tension of the surface. For, if we consider the effect of any two particles in a curved line on a third at an equal distance beyond them, we shall find that the result of their equal attractive forces bisects the angle formed by the lines of direction; but that the result of their repulsive forces, one of which is twice as great as the other, divides it in the ratio of one to two, forming with the former result an angle equal to one-sixth of the whole; so that the addition of a third force is necessary in order to retain these two results in equilibrium; and this force must be in a constant ratio to the evanescent angle which is the measure of the curvature, the distance of the particles being constant. The same reasoning may be applied to all the particles which are within the influence of the cohesive force; and the conclusions are equally true if the cohesion is not precisely constant, but varies less rapidly than the repulsion.

Law of superficial cohesion.

VII.

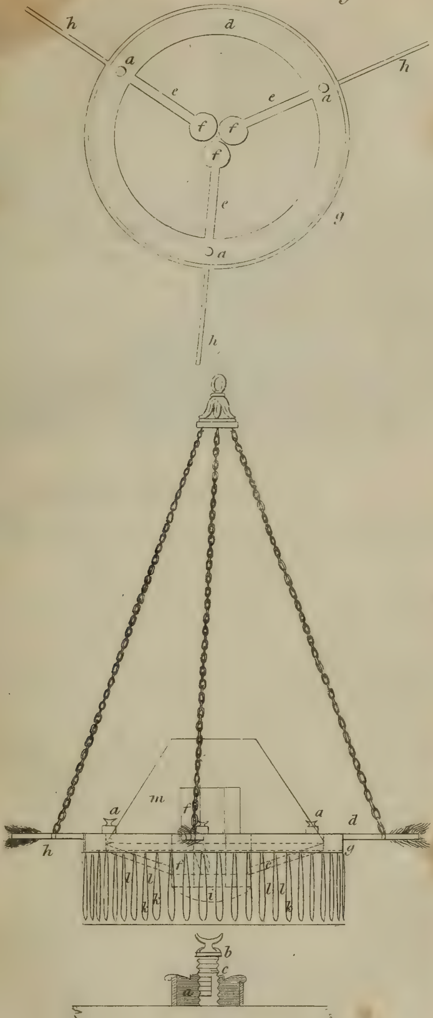
VII. *Cohesive Attraction of Solids and Fluids.*

Cohesive attraction of solids and fluids.

When the attraction of the particles of a fluid for a solid is less than their attraction for each other, there will be an equilibrium of the superficial forces, if the surface of the fluid make with that of the solid a certain angle, the versed sine of which is to the diameter, as the mutual attraction of the fluid and solid particles is to the attraction of the particles of the fluid among each other. For, when the fluid is surrounded by a vacuum or by a gas, the cohesion of its superficial particles acts with full force in producing a pressure; but when it is any where in contact with a solid substance of the same attractive power with itself, the effects of this action must be as much destroyed as if it were an internal portion of the fluid. Thus, if we imagined a cube of water to have one of its halves congealed, without any other alteration of its properties, it is evident that its form and the equilibrium of the cohesive forces would remain undisturbed: the tendency of the new angular surface of the fluid water to contract would therefore be completely destroyed by the contact of a solid of equal attractive force. If the solid were of smaller attractive force, the tendency to contract would only be proportionate to the difference of the attractive forces or densities, the effect of as many of the attractive particles of the fluid being neutralized, as are equivalent to a solid of a like density or attractive power. For a similar reason, the tendency of a fluid to contract the sum of the surfaces of itself and a contiguous solid, will be simply as the density of the solid, or as the mutual attractive force of the solid and fluid. And it is indifferent whether we consider the pressure produced by these supposed superficial tensions, or the force acting in the direction of the surfaces to be compared.

(The conclusion in our next.)

Improved Lamp by Count Rumford



From the Model of Mr. Arnold's Escapement.

Fig. 5.

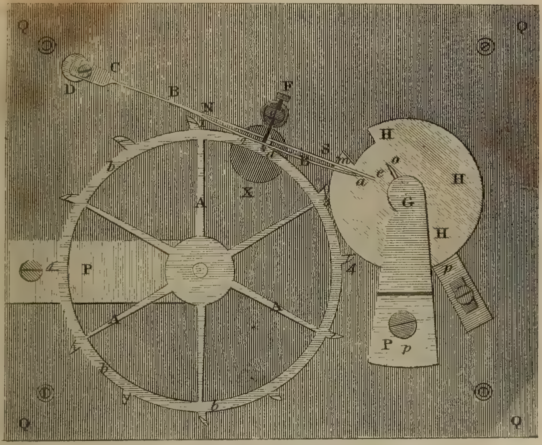


Fig. 10.

Fig. 7.

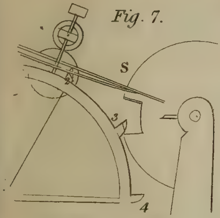


Fig. 6.

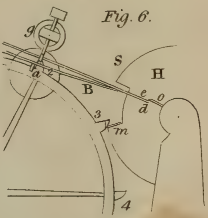


Fig. 2.

Fig. 1.

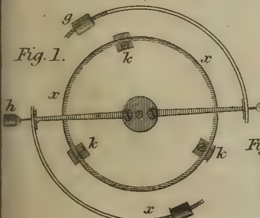


Fig. 9.

Fig. 3.

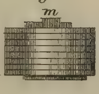


Fig. 8

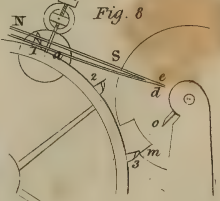
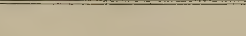


Fig. 4.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1806.

ARTICLE I.

Letter of Inquiry from a Correspondent, respecting the spontaneous Recovery of the Edge in dull Razors laid aside for a time; and a Postscript, shewing that Lavoisier has no title to the Discovery of the Modern Theory of Oxidation: with a Reply and some Remarks by W. N.

To Mr. NICHOLSON.

SIR,

Edinburgh, 10th May, 1806.

HAVING often remarked that a razor which by use had lost its edge, and was laid by, recovered it again when newly strapped; I have been in the habit for some time past of putting my razor aside for a short time when it became dull, with the view of improving its power of cutting. I have asked some experienced hair-dressers who are much in the habit of shaving, if they had ever noticed the fact, and received an answer in the affirmative: and on inquiring a few days ago of a surgeons' instrument maker of this place, whether he could tell me the cause, he replied that the same question was more than once put to him by the late Dr. Black, but that he was unable to give any satisfactory answer. If you should deem the observa-

A dull razor recovers its edge by being laid aside for a time.

The same fact noticed by others, and by Dr. Black.

servation worth notice, and are able to give me an adequate explanation, I shall be much obliged by your insertion of this note in some future number. Can it be owing to any chemical action exerted on the fine edge of the metal by the oxigenous part of the atmosphere, whereby its quality is so changed as to cause it to yield more to the strap than it did before?

Question as to cause.

I am, Sir, your constant reader,

E. D.

The editor reminded of his promise respecting Lavoisiers claim to the theory of oxidation. See our vol. xiii. page 84.

P. S. I was pleased to see your annotations on the notice of the late celebrated Lavoisier's posthumous collection of memoirs; and hope you have not relinquished your intention of vindicating the just rights of our own philosophers against the unqualified claims of that author. I subjoin the following remarks which I lately met with on oxidation. By an experiment of Mr. Homberg's, four ounces of regulus of antimony being calcined by a burning-glass for an hour together, were found to have imbibed and fixed seven drams of the substance of light.* Here the increase of weight by calcination is decisively remarked; though it is attibuted to a wrong cause. But Dr. Hales says, "if fire was a distinct kind of body inherent in sulphur, as M. Homberg and Lemery imagine; then such bodies when ignited would rarefy and dilate all the circumambient air, whereas it is found by many of the preceding experiments that acid sulphureous fuel *attracts* and *condenses* a considerable part of the circumambient elastic air."† And again he says, speaking of the weight which lead acquires by calcination, "that there is good store of air added to the minium, I found by distilling first 1922 grains of lead, from whence I obtained only seven cubic inches of air; but from 1922 gains, which was a cubic inch of red lead, there arose in the like space of time thirty-four cubic inches of air. It was therefore doubtless this quantity of air in minium which burst the hermetrically sealed glasses of the excellent Mr. Boyle, when he heated the minium contained in them by a burning-glass: but the pious and learned Dr. Nieuwentyt attributes this effect wholly to the expansion of

Lavoisier was not the first who observed the increase of weight in oxidation of metals. Homberg observed it in antimony oxided by solar heat. Hales found by experiment that the gain is from absorbed air.

He separated the air again from the minium.

* Bp. Berkley's Sirius, par. 169.

† Beg. Statice, 4th edit. p. 285.—Ibid. 289.

"the

"the fire particles lodged in the minium." I think I remember to have seen a communication from Dr. Priestley in one of the early numbers of your 8vo. series, wherein he says, that when formerly in company with M. Lavoisier at Paris, he informed Mr. L. of his having obtained a very pure kind of air from red lead, which was what he afterwards named pure or dephlogisticated air.

With regard to *oxidation*, therefore, and its cause, M. Lavoisier did not discover; he only confirmed and extended the fact. With respect to the *theory of combustion*, I am sure you will do our countryman Hooke justice. I shall only observe that I do not think we possess any complete theory of that process; for all that we know is, that the pure part of the air combines with the body that is burnt, and forms various compounds according to the nature and composition of that body: the phenomena of flame as yet are unaccounted for. Nearly the same may be said of the claim M. Lavoisier makes to the *theory of respiration*. All the great facts tending to an explanation of that function have been the successive discoveries of Boyle, Lower, Black, Priestley, and Crawford; but nothing in the shape of a satisfactory theory has yet been produced. M. Lavoisier's claims, therefore, in this case, as far as concerns the *facts*, are notoriously the discoveries of others: and as to what regards the *theory*, it is unnecessary to contest with him the right to a thing which has no existence. The truth is, Sir, M. Lavoisier *discovered* very little, but he *improved* and *generalized* a great deal. No one will question or deny his high merits in that point of view, which certainly affords the surest proof of a philosophical genius, inasmuch as the person who constructs the building ranks higher than the mere workman who prepares the materials. But M. Lavoisier in assisting to prepare the materials was too apt to forget his fellow-labourers in the work: and it now appears that he lays claim to the construction of a building which has not yet been raised. An impartial and minute observation of the progress and evolution of any science, will convince us, that what may be called "great discoveries" are things of very rare occurrence; and that, perhaps, Newton is the only person who in the race of discovery has distanced all the rest of mankind.

Lavoisier did not discover, but only confirmed the fact.

The theory of combustion belongs to Hook;

—supported by facts from Boyle and others.

 Reply.

Reply. W. N.

The fact that razors, &c. recover their edge by laying by, admitted.

That razors and other fine-edged tools when dull, do recover a certain degree of sharpness after being kept for a time, has, I believe, been frequently noticed. I think the fact can scarcely be considered as doubtful; but that it is requisite to strap the instrument in order to produce this acuteness, has not, that I know of, been before remarked.

The editor ascribes it to oxidation and the peculiar nature of steel. Some account of Damascus steel.

My former meditations on this phenomenon have induced me to ascribe it to oxidation modified by a peculiarity in the nature of steel. In the quarto series of this Journal, vol. i. p. 468, I gave a short account of a Damascus sabre, with observations and experiments on the method of making that kind of steel; which has a wavy or fibrous appearance on its surface, called the water, and is valued for its rough cutting edge and its tenacity. It was there explained that the water is produced by the action of an acid upon the metal; of which the minute aggregate parts are some iron and some steel; the former being more readily corroded than the latter. I likewise mentioned a method I had, much to my own profit and satisfaction, used for ascertaining the texture of steel; by applying diluted nitrous acid to its clear surface; the effect of which is to shew the respective veins or imperfect mixtures of iron in the steel,—because these appear of a lighter colour than the steel itself, which loads its solvent with plombage or carburets of iron. On the present occasion I must add, that I never found any specimen, even of the finest cast steel, which did not exhibit a very considerable mixture of parts, some more and some less steely.

Method of examining the uniform texture of steel by an acid.

All steel is probably the same as Damascus steel, but fine grained.

I strongly suspect that these facts may be applied to the case before us. I would assume the position, that the same irregularity which the acid shows us in steel, does also exist among its very minute and perhaps invisible integrant parts; or that all steel has the property of Damascus steel with regard to these parts. Whenever a razor is brought to a keen edge by grinding, setting, and strapping, the fretting powders or the substance on which it is thus rubbed convert it into a very fine toothed saw. By employing this edge against a soft substance the teeth are rubbed, down and the edge, though still thin, becomes much less notched than before: the razor is then dull, and requires to be

The fine teeth of a cutting edge are worn down by use;

be revived by setting or strapping. Instead of this process, however, let it remain exposed to the effects of the atmosphere. If it be thus left for a sufficient time, it will become oxided or quite rusty: but before this happens, it is reasonable to admit that an invisible effect will be produced on its thinest part or edge; or in other words, that it will be corroded by the joint action of the water, carbonic acid, and oxigen of the atmosphere. We have seen in the sabre that the iron is corroded before the steel; and in one of fine Damascus structure the iron parts will be oxided and the steel parts left,—that is to say, the edge will again spontaneously become toothed, and the razor of consequence sharper than before.

—setting reproduces them:—but if laid aside the razor will rust, in time; particularly on the edge;—and that unequally, so as to reproduce teeth and restore the edge.

The use of strapping is to render the edge finer than that which comes from the hone. If we imagine the atmospheric oxidation or corrosion to give a somewhat coarse edge (which it must do if it revives an edge that was too dull for the strap), the use of the strap mentioned by E. D. will be to do the same in the present case. Or perhaps it may be of use to carry away the oxide with which the chemically formed notches will be clogged.

Strapping may render the edge finer, or clear away the oxide.

It is with great satisfaction that I find the delay respecting my promise in the place cited (xiii. p. 84.) so well compensated for in the postscript to E. D.'s letter. Confinement by illness, and the pressure of many objects of business by that means increased, were the causes of that delay. I fully agree with him that all our theories of chemistry, viz. of heat, combustion, and the affinities, are crude, hypothetical, and unworthy of the importance which is attached to them; though the discoveries of facts relating to each are such as do honour to the sagacity of the great men who have laboured in those several departments of research. I hope to be able shortly to state so much of the history as may extend the observations of my ingenious correspondent in settling the claim of M. Lavoisier to which he has attended. The mischiefs as well as the merits of generalizing are intitled to the serious attention of philosophers; and the integrity of any claimant, whose demands are not well founded, can only be vindicated by a supposition that he did not know the true owners of that which he may profess to have found. M. Lavoisier well knew the claims

Concerning the claims of Lavoisier.

All our theories of chemistry imperfect.

General analogies are as often detrimental as beneficial to science.

M. Lavoisier did attempt to obtain honor

from the discoveries of others.

claims of Hales, Cavendish, Priestley, and some others, to whose discoveries he himself and his numerous party at Paris have by direct as well as indirect means endeavoured to establish a title in his own name.

II.

Observations and Experiments on Galvanism, the Precipitation of Metals by each other, and the Production of Muriatic Acid. By CHARLES SYLVESTER, *Esq.*

To Mr. NICHOLSON.

SIR,

Theory of Galvanism.

IN October 1804, you did me the honour of inserting in your valuable Journal, some experiments and observations, tending to prove that galvanism was strictly a chemical process, depending upon a double affinity, existing between the water and the metal; the metal being supposed a compound electricity of metallic base, and the water a compound of oxigen and hydrogen.

Some remarks on the theory of H. B. K.

I observe in your last Journal a new mode of theorizing on those appearances by Mr. H. B. K. As it is my intention in this paper to give you some new experiments which have for some time occupied my attention, I shall not at present examine Mr. H. B. K.'s hypothesis, having only had time to observe that his opinions are very singular and very liable to objection. I beg leave however just to contradict several assertions made by Mr. K. I have no objection to his convincing the world of the absurdity of modern chemistry; but I would recommend him to bring forward facts for that purpose, without which every opinion must fall into nothing.

Some errors in his facts pointed out.

In page 224. No. 52, he says, that with water the wires were much calcined. Now it is right that Mr. K. should know that only one wire, viz. that which comes from the zinc end of the pile could be calcined; for invariably deoxidation would be going on at the opposite wire if the oxide of a metal were present. Mr. K. tells us that the wires are calcined by an acid; and to prove it, he introduces the same wires which were calcined with water, into a solution of potash, and asserts they were not in the least acted upon. This is certainly not true. The wire coming from the copper end of

of the pile, as in the experiment with water, is not acted upon; but the other is oxided in a much greater degree. With my large apparatus (100 plates 92 square,) when I made use of brass wires in a solution of potash, the liquid in the course of ten minuets became perfectly green with the copper of the wire, and was rendered at the same time turbid with the suspended oxide of zinc. I shall not at present say more on this subject, as I mean in a future communication to give some experiments on the action of galvanism upon bodies dissolved in water.

The experiments with animal fibre (instead of wire) which affords no gas, explained. The waters so connected are considered as one fluid.

Mr. K., in his last paper, has made some assertions equally destitute of foundation. I am surprised at his giving himself the trouble of attempting to produce gas by using animal and vegetable substances instead of metal. The moisture contained in these bodies connected the liquid of the cell of the trough with the glass of water, making them the same as one vessel. If Mr. K. had looked into the cell where one end of this connecting substance was placed, he would have seen the gas upon the copper plate at the copper end, and the zinc oxided at the zinc end: so that the cells at the opposite end, the moist substances, and the water where these substances terminated, were the same as one vessel, while the terminating plates of the trough or pile performed the office of the metallic wires.

Longer wires do not afford more gas, &c.

Mr. K. asserts that he had more gas when the wires were longer. This I deny. He afterwards observes that an iron wire at the silver end of the pile " became brittle." If he made use of a trough, and one end of the wire was immersed in the liquid of the cell, it would be oxided as far as the liquid touched it, and might so far be brittle, but no farther.

Interesting experiments of precipitation of one metal by another.

I have been lately engaged in a very interesting department of chemistry which has been very much neglected, viz. the precipitation of one metal in solution by another in its solid form. The simple action of the piece of metal upon the oxide in solution is instfficient to explain a number of very curious phenomena attendant on some of these processes. In the arbor dianæ we see the appearance of vegetation; and in the experiment with acetite of lead in a phial with a piece of zinc, we observe a bundle of fine filaments of metallic lead reaching to the bottom of the bottle. If a thin

The precipitation in the arbor dianæ takes place at the extremities of the branches.

Experiment of silver precipitated by zinc on the face of a glass plate in ramifications.

thin coat of a solution of nitrate of silver be laid upon a piece of plain glass, and in the centre of this be laid a bit of zinc wire, in a little time a beautiful tree of silver will appear as if growing from the wire. If the process be observed with a magnifying glass, the ramifications of silver will be seen to increase by the progressive reductions of silver at their farthest extremity from the zinc; a clear proof that the oxide of silver does not owe its reduction to the zinc, but to something which exists at the point where the increase is taking place. and which I shall prove by the following experiments to be dependant upon the principles of galvanism.

Variation of the last experiment. One half of a glass plate was coated with sol. of silver, the other half with diluted muriatic acid: zinc wire touched the latter fluid, and platina wire the former. When these wires were united at their ends, the silver tree grew from the platina, but ceased when they were disjoined. The act of precipitation was galvanic. Explanation: If silver had not been in the solution the platina would have developed hydrogen. This is supposed to have been transmitted from the place of oxidation to the place of reduction and precipitation

In the above experiments with the glass plate I made this variety: I coated one half of the glass with nitrate of silver and the other with dilute muriatic acid, so as to touch each other. I then laid one end of a platina wire upon the nitrate of silver, while the other rested on the table, a piece of zinc wire was similarly situated upon that side of the glass covered with the dilute acid: upon uniting those ends of the wires upon the table, I soon had the satisfaction to see a beautiful tree of silver grow from the point of the platina wire. This effect ceased as soon as the ends of the wires were separated. If instead of nitrate of silver the whole of the glass had been covered with the dilute acid, bubbles of hydrogen would have been given out at the platina wire. In this experiment the water is decomposed, the oxigen combines with the zinc, while the hydrogen enters into some combination by which it is invisibly carried across the liquid to the platina wire, where the hydrogen is liberated in its gasseous form. It is easy to observe, that when the platina was in contact with the nitrate of silver, the hydrogen was employed in reducing the silver to its metallic form. When a piece of zinc simply is laid upon the coating of nitrate of silver, the zinc in the first instance reduces the oxide of silver immediately in contact with it: the silver and zinc have now become a galvanic combination, and the remainder of the process is carried on by means of galvanism. The zinc is now oxided by the oxigen of the water, and the silver is reduced by the hydrogen. In the experiment with zinc and the solution of lead, the same explanation holds good. As soon as the least portion of metallic lead is produced by the zinc, a decomposition

of

of water takes place; the oxigen uniting with the zinc, while the hydrogen is invisibly diffused through the liquid, reducing the oxigen of lead upon the metallic lead already found.

To prove that the whole of this effect can be produced by galvanism without the zinc touching the solution of lead, I made the following experiment:

The glass tube *AB*, Plate III. Fig. 1. had a piece of thin bladder tied at *B* so as to hold a liquid when filled; the tube was filled with acetite of lead; the vessel *D* was of zinc partly filled with dilute muriatic acid, the wire (passing from *C* through a cork at *A*, and coming down so near to the bladder as not to pierce it) was of pure platina. The bladder it is plain separates the zinc and the acid from the acetite of lead. The wire was for some time unconnected with the zinc vessel, but no change whatever took place in the tube; but as soon as the contact was made at *C*, metallic lead began to be formed at the point of the platina wire. I obtained in this way about six grains of metallic lead. When the tube was filled with the dilute acid instead of acetite of lead, hydrogen gas was given out at the platina wire; so that it seems the bladder itself could not prevent the passage of the combined hydrogen.*

Experiment by vessels in which the dilute acid was separated from a solution of lead by bladder.

The lead was precipitated as in the case before stated by platina.

When lead was not present hydrogen was developed.

When acetite of lead is decomposed by galvanism, the acid is set free, and I believe it is the case with all metallic solutions under the same operation. For the sake of not occupying too much of your room as well as my own limits, being confined, I must defer for a future communication more minute observations upon these experiments. By the favour of inserting the above in your next Journal, if you are not already provided with something more important, you will much oblige

In thus decomposing the metallic solutions the acid is set free.

Your most humble and obedient servant,

CHARLES SYLVESTER.

Sheffield, *May* 14*th*, 1806.

P. S. I have just made the experiment announced by M. Pachiani on the discovery of the muriatic base, and repeated

Pure water treated with platina wires in

* It is very probable that the electricity present on these occasions combines with the hydrogen when the oxigen combines with the zinc, and that it is invisibly transmitted through the liquid to the platina wire.

 with

the apparatus last mentioned, indicated muriatic acid and an alkali.

without success by the galvanic society at Paris. I made use of water which was not altered by nitrate of silver. I took a tube, secured at one end by a bladder, into which I introduced pure water. At the other end was a cork, through which a platina wire was passed nearly to the bottom of the tube. I then set the tube in a wine glass containing also pure water, into which I introduced another platina wire, the end of which came under the end of the tube, as near the bladder as possible. The wire in the tube was connected with the zinc end of the trough, that in the glass with the copper end; after the process had continued an hour, I put the liquid in the tube to the test of nitrate of silver; and when I had a sufficient precipitate to indentify the presence of muriatic acid, the liquid in the glass contained an alkali, which I suspect to be ammonia. I hope in my next to speak more fully to this. If, as Mr. Peel has asserted in the Philosophical Magazine, this alcali should be soda, what an important field will be opened to the chemist and the naturalist! If a galvanic trough of moderate size be capable of generating muriate of soda from water, the same effect must in some degree take place when two metals of different degrees of oxidability are immersed in this liquid. Hence the water used in the economy of life and in other situations, must be frequently undergoing this change. The muriate of soda, thus generated, is carried by the rivers into the ocean, from whence it cannot return by evaporation: need we then wonder at the saltness of the sea?

Query whether the bladder affected or caused these products? W. N.

III.

Of the Utility of the Water Ram; *by* M. Jos. Mongolfier, *Demonstrator to the Repository of Arts and Trades, Paris.** *With Remarks by* W. N.

The raising water by men or horses is expensive.

It is frequently necessary to raise water to considerable heights when we are precluded from employing the strength of men or animals for this purpose, because these would be too expensive. Accordingly, recourse has been had to streams of water, or the effects of fire, to supply the power required.

* Translated from Sonini's Journal, No. V. Feb. 1806. p. 331.

The

The force of water has hitherto been applied only by means of wheels, or of pumps, more or less complicated in their construction; and, notwithstanding the improvements made in machines of this kind, the cost of their erection has been so great, and the supply they have furnished so small, that princes and great towns alone have been able to avail themselves of them. A private individual, even of considerable wealth, cannot afford an hydraulic machine: and agriculture, in which irrigation is so frequently necessary, has derived no assistance from such engines.

Water may be employed as the moving power: but the machines are, in many instances, too expensive.

With regard to steam engines, as their construction will scarcely be deemed less complicated than that of hydraulic machines moved by wheels, and as fire is often more expensive than a stream of water as a first mover, they can be used for raising water with advantage only in large works, and where coal is plentiful.

Steam engines too dear, unless for large works, and where coal is cheap.

These were the reflections which led me to seek after other means of applying the power with which we are furnished by nature in falls of water, for the purpose of raising, at little expence, a part of the same water to any height that may be required.

Investigation of simpler means,

In this inquiry economy being my object, it may be presumed that I turned my attention entirely to the most simple means I could invent; for by simplicity alone could I hope to succeed. Consequently I was obliged to give up the system of wheels, and of pumps, in which so many have exerted their abilities to little purpose.

because most economical.

What indeed can we expect from the common hydraulic machines? They display a complicated and of course expensive assemblage of materials of various kinds, the cost of which is frequently increased, as well as their effect injured by the strangeness of their figure. Is not the machine of Marly in its kind the *opus magnum* of mechanists, a kind of monstrosity, whether we consider the multitude of its parts, the immense sums of money it cost, or the small quantity of water it raises?

Defects of hydraulic machines.

Machine of Marly.

The better to avoid the defects inherent in the nature of these machines, I chose one founded on principles totally different, principles that completely exclude the defects I wished to avoid. An exhibition of these principles, and the

The water engine of the author.

theory of the action of this machine, are not my object in the present paper; in which I mean only to make known their practical results, in such a way as to enable any one to form a sufficient judgment of the effects produced by this machine, and avail himself of the advantages it offers. A description of the machine itself, and an account of some of the trials made with it, will be sufficient to show, that its simplicity gives it a decided superiority over others, from the less original expence it demands, and the greater supply it affords.

Advantage of simplicity in machines.

This double advantage obtained at the same time is very evident. In fact, simplicity is the most estimable quality of a machine; at the same time that it renders it cheaper in the first cost, and to keep it in order. It is better, because every useless piece not only adds to the expence, but is detrimental to the end to be obtained. The simplicity of a machine is therefore an argument in its favour with men of sense; and we may reasonably expect greater effect from a simple engine than from one of greater complexity.

What is meant by the effect of a machine.

It may not, perhaps, be amiss to explain what I mean by the effect of an hydraulic machine, as it may lead to a more accurate judgment of the true value of my engine, called the Water Ram (Belier), and other machines.

I have premised, that for raising water we must employ some power which is furnished by different agents. The ram is worked by that part of the water that falls.

Power of a fall of water.

It is a simple fact, of which no one can be ignorant, that falling water has only a determinate power, which is proportionate to the mass of the water, and the height from which it falls: accordingly, if I would express the power I have to employ in numbers, I multiply the mass of the falling water* by

Simple means of estimating the moving power of water.

* The means we have of measuring accurately the quantity of water flowing from a spring or in a river, are not of a nature to be employed with facility by every one: but we may have recourse to approximations, the results of which will always come much nearer the truth than would be supposed from the notions commonly formed on such subjects, and will often be fully sufficient for my purpose. For example, if it be wished to be known how much water flows through an aperture under a certain pressure, it will

by the height it passes through, and the product of these two numbers is the expression of the power. Thus I may know with precision the quantity of the power I have at my disposal.

As I give the name of *power* to the product of the mass of water multiplied by the height of its fall, I likewise give the name of effect to the product of the mass of water raised, multiplied by the height to which it is carried. This is the effect or useful product of the machine.

Author's use of the terms *power* and *effect*.

This second quantity can never be equal to the first, because in the motion of the machine a part of the power is always expended [in generating velocity, and] in overcoming friction, and in useless movements: but we may bring them considerably nearer to an equality, by diminishing, as much as possible, the last two impediments of which I have been speaking, and this I have in part effected in the water ram.

The effect is never equal to the power producing it.

Thus to estimate the value of a machine, we must find the ratio of the power expended, or of the mass of water that falls multiplied by the height of the fall, to the effect produced, or the mass of water raised multiplied by the height of its ascent. The comparison of these two quantities gives the true measure of the excellence of the machine.

Ground of the comparative estimate of machines.

will be sufficient to measure the aperture, and the height from the middle of the aperture to the surface of the water. By multiplying the surface of the aperture by the velocity the water would acquire in falling from the surface to the middle of the aperture, and taking two-thirds of the product, we shall have nearly the quantity discharged. If we had a brook or a rivulet to measure where there was no fall, we may observe the height to which the water would rise against an obstacle opposed to the stream, such as a slip of wood two inches wide and half an inch thick, and calculating the velocity as in the preceding case, multiplying it by the surface of a section of the river perpendicular to the current, and this would give the mass of the flowing water.

The velocity of the water may also be better estimated by that of any floating body, following it a little time with a watch in the hand. If we have a small spring to measure, and a reservoir at our disposal, it would be a much more certain method to calculate the capacity of the reservoir, and observe the time it took to fill. (These last give rough estimates only. N.)

The

Machine of Marly.

The machine of Marly, when first constructed, appears to have produced $\frac{1}{24}$ of the power expended, so that $\frac{23}{24}$ of its power were (chiefly) misapplied. This misapplied power has been injurious to it, and the wear it has occasioned has reduced the effect to $\frac{1}{40}$, $\frac{1}{60}$, and at length even to $\frac{1}{200}$.*

Hydraulic machines in general lose 9-10ths of their power.

It would be difficult to exhibit the results of other similar hydraulic machines with wheels and pumps, because they are not common, and are often erected under circumstances where it is not easy to make the proper calculations; but it is certain that, with all possible care in the execution, they usually produce less than one-tenth of the power employed. They are not very productive therefore; but this is not the only reason for their being so rare. In fact they are not applicable to a number of falls of water, small in quantity, or of little height; and their expence includes not only the cost of the machinery, but of buildings to defend it from the weather, which renders them very costly.

Why they are not more common.

The effect of the water ram equal to 3-4ths of its power, and at least $\frac{1}{2}$.

A water ram, made with care, may produce $\frac{75}{100}$ of its first power, as I have seen; but in general it gives only $\frac{60}{100}$. Indeed I engage only for $\frac{50}{100}$ or $\frac{1}{2}$: I mean, that if you would raise water by it 100 feet with a fall of 5, only a fortieth part of what falls will be raised to that height. If the fall were 10 feet, a twentieth part of the water would be raised: if the height were 200 feet, and the fall 5, only an eightieth part; and so in all other cases.

I shall hereafter relate the experiments from which I deduce these results.

Superiority of the water ram.

Thus we may admit, that the water ram generally gives $\frac{50}{100}$ of the power it expends. Its effect therefore is five times as much as that of the common hydraulic machines, under the same circumstances; yet it is far from being perfect, as appears from the rules I have laid down for judging of the merits of a machine. It may be rendered more effective, as I have

* The part of the Seine that passes the machine at Marly is nearly equal to 300,000 cubic inches, and falls about four feet and half. The moving power therefore is $300{,}000 + 4{\cdot}5 = 1{,}350{,}000$. The quantity of water raised was 120 hydraulic inches, and the height 475 feet. The power produced therefore was $120 + 475 = 57{,}000$, or $\frac{1}{24}$ of that expended.

already

already observed; but the care that it would require to make it so, would occasion it to be too costly in some instances for the purpose for which it is intended: there are circumstances, however, in which it would be worth while to purchase water at this price.

I recommend, therefore, to all who have falls or streams of water at their disposal, the power of which may be employed for raising water, either for the supply of houses or manufactories, or for irrigations, to employ the *water ram*. There are many situations in which it may be of the greatest utility. The great expence of other machines has in some measure prevented on frequent occasions the use of water. Farmers have never even thought of applying it in husbandry; but in future they cannot avoid considering this operation as very easy, and calculating the great advantages they may derive from artificial irrigations.

Its use in husbandry.

The construction of the water ram even enables them to raise turbid and muddy water, which is so useful as manure; and it requires but little care to prevent such water from doing the least harm to this machine.

Muddy water may be raised by it.

The following is the description of a water ram. Plate III. Fig. 2. represents it placed before a dam *A*, constructed so as to confine the stream of water, and oblige it to pass through a cone *B*, adapted to a long tunnel *CD* of iron or copper, connected with the masonry of the dam, and placed on pieces of wood which support it throughout its whole length.

Description of the water ram.

The depth of water above the cone *B* is supposed to be four feet, and the length of the tunnel *CD* twenty-four feet.

To the extremity of the tunnel *D* a piece of copper or iron, which I call the head of the ram, is fitted. In this head are two apertures; one, *O*, which the valve *G* shuts by rising; another, *I*, which the valve *K* closes by falling. The motions of these valves are guided by sockets through which the stems or axes passing through their centres are made to pass.

When the aperture *O* is open, the water issuing from it is dispersed on all sides. *I* is covered with a kind of bell, *R*, in the side of which is a hole, with a pipe *LU* fitted to it, which rises as high as the water is intended to be conveyed.

The piece of wood on which this machine is placed reaches under the head of the ram, and is fixed on a solid wall, which

is

is built at H, and receives the support of the head of the ram, so as to render the whole of the machinery as firm as possible This is easily accomplished by laying H with very large stones, or by placing them against immovable obstacles.

Manner of its acting.—An horizontal column of water is suffered to run out thro' a valve, which shuts when a certain velocity is acquired. The momentum then drives a portion of water through another valve to the elevation required.

I shall now speak of the action of the machine.

If the valve G open, the water filling the long tunnel $BCDO$ will issue out, and disperse itself with a velocity continually increasing; for this water will fall, and it is well known that falling bodies acquire an increase of velocity.

Now at a certain velocity the force of the current will raise the valve G, and the orifice O will be closed. The whole of the cylinder of water included in the long tunnel $BCDO$ will then be suddenly stopped; but the active force with which it is impelled cannot be annihilated: this therefore will exert a pressure against the whole of the inside of the tunnel, and as this is very strong, the pressure will impel a certain quantity of water through the aperture I, which the valve, raised by the water itself, leaves open. Consequently water will enter into the bell R. The active force existing in the cylinder of water being thus expended, the pressure, to which the valve G had yielded, will cease to exist, and the elasticity of the metal, assisted by the weight of the valve, will cause it to descend again. The aperture O being then open, the water will begin again to flow, and these operations will be repeated as long as there is any water before the dam.

The effect is produced by alternations

This succession of actions, which has taken me so long to describe, passes in a very short space of time; frequently in less than half a second, sometimes in three seconds, according to the dimensions and conditions of the machine: but be the time what it may, all that I have said actually takes place, beside other events, that it is not necessary to mention here.

—from half a second to three.

It is obvious then, that every time the stopping valve O closes, a portion of the water which was in motion will pass into the bell. This will soon be sufficient in quantity for the extremity of the ascending pipe to be immersed in it, and then the air over the water will have no communication with the atmosphere. If the machine continue its action, the water entering into the reservoir will compress this air, and this air, reacting on the surface of the water, will force it to ascend in the pipe LU, to the greatest height that can be given to it.

The water may be raised to any height.

In

In fact, were this pipe 1320 feet long, so that the pressure exerted by the water in it would be forty times that of the atmosphere, the air would be condensed by this pressure into a fortieth part of its usual bulk, and in consequence the ascending valve K could not be raised without a pressure at least equal to that of the air: but at the moment the stopping valve closes, the cylinder of water in motion cannot be stopped without imparting its active force to a portion of the water, which has no easier way of escape than by the aperture I; accordingly the valve K will be raised, and a portion of the water will enter into the reservoir. This, it is true, will be less, in proportion as the height of the ascent is greater; but it will nevertheless contain the greater part of the active force exerted by the motion of the cylinder of water $BCDO$.

A quarter of a mile perpendicular, for instance.

The example I have taken of a pressure of 1320 feet has been verified by experiment, and all that could have been expected in it actually took place. This pressure is considerably greater than any that usually determines the height to which attempts have been made to raise water, so that the water ram is applicable to every possible case of ascent, a circumstance that could never be accomplished by wheels or pumps.

This proved by experiment.

This description must be sufficient to show, that the ram is liable to very little friction, and that in consequence it will last much longer than other machines. If the parts be made as strong as the calculations of the resistance requires, it may be presumed it will produce a very profitable effect for a great length of time without much alteration.

The engine is liable to very little wear.

The following is an account of the two experiments, which I promised, to show the value of the ram.

Estimate of the expenditure and produce of the water ram constructed at the bleaching works of M. Turquet, near Senlis.

Force expended and effect produced by a water ram. Exp. I.

The ram is seven inches and half in diameter; the height of the head of water above the stoppage valve is three feet two inches.

The height to which the water is raised is fourteen feet two inches above the same point.

The water that descended in a hundred strokes of the ram, which took place in three minutes, was 1987 English quarts:

Ten parts of water raised six by its fall. Height 11 feet.

The water raised .... 269 English quarts.

The force expended therefore was $1987 \times 3\frac{1}{6} = 6293$.

That obtained, or the effect ...... $269 \times 14\frac{1}{6} = 3811$.

Consequently if I represent the first of these forces by 100, they will be in the proportion of 100 to 60. That is to say, the force received was $\frac{6}{10}$ of the whole the machine could employ.

An engine $7\frac{1}{2}$ inches diam. raised 500 hhds. a day above 11 feet.

In a second experiment, made somewhat differently, it was $\frac{61}{100}$ of the force applied.

This ram delivered at the height of fourteen feet two inches (say 11 feet) a stream of water equal to 20 hydraulic inches (pouces de fontanier), and in twenty-four hours working would raise near 130,000 quarts to that height.

Exp. II.

Estimate of the expenditure and produce of a water ram two inches in diameter, placed in my own garden, rue des Juifs, No. 122.

The fall of water, which I have procured artificially for want of a natural stream, is $7\frac{1}{2}$ feet.

By a smaller engine 10 parts raised $6\frac{4}{}$. Height 50 feet.

The height to which the water is raised is that of my house, or 50 feet.

The water expended in four minutes I find to be 315 quarts; that raised 30 quarts.

Accordingly the force employed is $315 \times 7\frac{1}{2} = 2362$.

That received .... $50 \times 30 = 1500$.

This engine of 2 inches would raise 43 hhds. a day 50 feet high.

They are therefore in the ratio of 100 to 64. Consequently the produce is $\frac{64}{100}$ of the expenditure.

This ram therefore is capable of delivering 10,800 quarts of water a day, at the height of 50 feet, provided the current be the same throughout that period.

Another water ram for streams.

The figure of the ram I have given is adapted only to falls of water. I have contrived another for streams, but this necessarily costs more money, though it may frequently prove very useful. It would take up too much room to describe the construction of this ram at full length, besides it must be varied according to the situation; and I would undertake it only for persons who, having occasion for such a machine, should acquaint me with the rapidity and magnitude of their stream of water.

I furnish only the part, which I call the head of the ram, or that

that included between D and H, with exception of the ascending pipe. I make all the parts, that form this head, of a proper strength; but as the whole requires to be in due proportion, it is necessary to point this out to those who would make use of the ram, and to apprize them of the precautions requisite to the success of the machine.

The dam, as I have supposed at R, is a very common structure. Falls of water are applied to some purpose or other, almost every-where, and this arrangment is always requisite. If a person have not such a dam, and wish to avail himself of a rapid stream to procure a fall, one might be constructed in the common mode, without any difference on account of the ram, except a place for fixing the tunnel BD in the part B.

A grating should be placed before the cone.

It is advisable to place a grating QT before the cone leading to the tunnel, to stop the greater part of the filth brought down by the current. This grating should not be very close, as it is intended to stop only the weeds and bits of wood that would be liable to prevent the valves from shutting perfectly. Mud and sand could be of little or no injury, because they are continually carried away by the stream O.

Body of the ram.

The body of the ram, BCD, should be throughout its whole length equal in diameter to the entrance D of the head of the ram. All irregularities, whether from swelling out or being narrowed in any part, should be carefully avoided, as they would greatly diminish the velocity of the water. The interior surface ought to be as smooth as possible, to occasion less friction.

The tunnel should be made of iron, copper, or lead. I shall give the means of calculating its thickness below; but it ought to be capable of sustaining at least twice the pressure of the column of water ascending from the air reservoir.

The joints of the different pieces which form the body of the ram, must be made with great care, and strong. The slightest aperture would diminish the effect of the machine. There must be neither elbows nor alterations in the slope. With respect to the length of the tunnel, I will give proper directions to any one who wishes to have a water ram constructed.

I shall have given every necessary information respecting the

 fixing

fixing of a ram, when I have spoken of the pipe for conducting the water from the reservoir of compressed air to the place required: for what I shall say of its thickness will be applicable to that of the body of the ram, of which I have promised to speak, with this difference only, that the tunnel ought to be nearly double the thickness of the ascending pipe, paying regard likewise to the proportion required by the difference of diameter.

To the head of the ram I add a piece of this tunnel, to which the rest may be soldered. This serves as a guide for the diameter, which ought to be the same, as well as for the thickness, if the tunnel be made of the same material as the head of the ram.

Rule for calculating the thickness of the pipes.

Should this not be the case, the following are the principles, on which the thickness of the material employed should be calculated. If we know the thickness of a pipe of given substance in a given experiment, and the height of the column of water, the pressure of which it bore without bursting, we may by this proportion the thickness of the pipe to be made to the pressure it will have to sustain, and the diameter given it.

For example, we know that a leaden pipe a twelfth of an inch thick and an inch in diameter has supported a column of water of 50 feet; and if we would know the thickness required to support a column of 100 feet with the same diameter, we must multiply the preceding thickness by the ratio of 100 to 50, that is by 2. If we would alter the diameter and not the height, we must multiply the preceding thickness of the ratio of the diameters; so that if the diameter were increased to 4 inches, the thickness must be $\frac{4}{12}$ of an inch. If the pressure and the diameter be both increased, the thickness must be increased in the ratio of both.

Nothing can be more simple than this calculation; and to avail ourselves of it we need only know the pressure that pipes of different substances have sustained with a given diameter. This the following table will show:

Table of the strength of pipes.

	feet.
A pipe $\frac{1}{12}$ of an inch thick and an inch in diameter, of copper, supported a column of water of	400.
A pipe of the same dimensions of brass of a good quality, about a fourth less, or	300.

The

	feet.
The same of sheet lead	50.
A pipe of cast iron, two inches in diameter, and 1-3d of an inch thick, at least	500.
A pipe of elder, an inch and half in diameter and two inches thick	30 or 40.

I have seen pipes of this kind however constantly supporting the pressure of a column of water of 110 feet.

Ascending pipe.

From these data it will be very easy to calculate the proper thickness of the ascending pipe according to its diameter, which should be the same throughout its whole length. If it be necessary to make turns in it, they should not be too sharp; for the water will move in them with more facility, the more gently the alterations in its direction are made.

It is obvious, that, when water is to be raised to a very great height, the ascending pipe does not require the same thickness throughout its whole length. The greatest thickness is requisite only for the lower part, the pressure diminishing as the column of water shortens; the thickness therefore may be gradually diminished also, calculating what is required at different heights according to the rule above given.

Having now given a sufficient description of this new machine, to make it known to those who may have occasion for such a one, it will be proper to speak of the expence attending its construction.

Expence of construction.

This, as may be supposed, is very variable, as affected by the circumstances of its situation. However we may say in general, that the expence is proportional, 1st, to the quantity of water to be raised; 2dly, to the height to which it is to be conveyed; 3dly, to the distance from the reservoir; 4thly, to the fall of water, as the expence will be greater in proportion as this is less.

One of 4 inches raises 147 gall. a day, 180 feet, with a fall of 4 feet 9 inches,

An example will give a more accurate idea of the expence of the machine. The object was, to raise to the height of 108 feet the greatest quantity of water possible with a fall of 4 feet 9 inches. For this purpose a ram of 4 inches diameter was employed, which expends nearly 11,500 cubic feet of water a day, and in that time raises nearly 240 cubic feet to the height required. The ascending pipe is 900 feet long. The ram

properly

and cost £200. properly so called cost 2400 livres [£100], and the other parts necessary to the machine about as much. Thus for £200 a quantity of water fully sufficient for all the demands of a large family, for cattle, and for watering a garden, is raised to the height of 108 feet.

Costs little to keep in order. To keep the machine in order cannot cost much; for all that is required is the renewal of a few leathers, or pieces of trifling value, and in cleaning it after floods or frosts.

Tried in various places. I believe I have given sufficient proofs of the utility of the water ram; and the facts I announce may be verified at several places, where they have been fixed both in France and abroad. Individuals therefore may now judge of the advantages they may derive from the machine.

Principles applicable to other purposes, I have spoken only of one kind of water ram; but it is easy to conceive that the principles in which a machine so different from any hitherto known constructed, are capable of much more extensive application. For instance, water different from that which runs in the ram may be raised by it; that is to say, by means of a fall of water, we may draw water out of the bottom of a well or of a mine, as with any other engine; and from the simplicity of the ram it would have a great advantage over others generally employed for this purpose. But I shall not turn my attention to other applications of the principles of the water ram, till I have derived all the advantage possible from the application I wish to make of it to falls and streams of water.

—as raising water from mines or wells.

Remarks. W. N.

The water ram was proposed some years ago by Mongolfier, The engine described in the preceeding memoir as invented by M. Mongolfier was the subject of much attention about nine years ago, and a patent was then taken out for it in this kingdom. That part of the contrivance which is due to this ingenious experimental philosopher (so well known by his inventions in aerostatics), consists in his causing the operation of raising water by its momentum, first acquired by falling through a pipe, to be performed without attendance.

—and executed in Cheshire, much earlier, at the seat of P. Egerton, Esq. An engine of this kind, not acting spontaneously, was executed thirty-four years ago at Oulton in Cheshire, the seat of Philip Egerton, Esq., and is described by Whitehurst in the Philo-

Philosophical Transactions for 1775. It consists of a pipe $1\frac{1}{2}$ inch diameter and upwards of 200 yards long, proceeding horizontally from a spring eighteen or twenty feet above the level of the kitchen of the house, where a branch of the pipe terminates in a cock for supplying the domestic purposes of the family. The horizontal part of the pipe has a valve to admit the water, but prevents its return. After passing this valve it enters an air vessel similar to that described in the preceding account, and proceeds from thence by means of another pipe upwards to the brewery and other offices. Whenever the kitchen cock is opened to draw water, the whole column of 200 yards is set in motion, and upon shutting the cock, its momentum drives a portion of water through the valve into the air vessel, and thence upwards to the place of its destination.

Mr. Whitehurst does not say to what height it is thus conveyed, nor the quantity raised each time of opening and shutting the cock.

It is certainly of importance to know what may be the true value of the water ram; and as M. Mongolfier has left some parts of his account defective as to this object, I shall take the liberty of making some additional remarks. Introduction to some remarks.

P. 104. *this water will fall.*] It is not exactly the case that the water in the pipe moves by its fall. It is put into motion by the pressure of the head, and would acquire a velocity equal to that of a body that had fallen through the whole height from the surface of the flood (impediments of friction, eddy, or obstruction in the pipe excepted). I do not know that any one has examined the theory of this engine. The general proposition may be made thus, and I hope some of my correspondents will investigate it: The theory of the water ram has not been published.

Proposition.—Given the height of an head of water and the height to which a part of the fluid is required to be elevated by the water ram, it is required to determine the diameter and length of the pipe with the dimensions of the other parts of the engine, and also the number of strokes per minute which shall afford the greatest possible quantity of water by a (given) long-continued action. Proposition to be solved.

P. 105.

It is probable that the water ram would not be durable under high pressures.

P. 105. *the pipe* 1320 *feet long.*] This column of water which answers to more than 40 atmospheres, would, I apprehend, greatly exceed what in actual practice could be trusted either to the water ram or any other hydraulic engine. Bramah's pressure engine, described in our first volume of the quarto series, supports between two and three hundred atmospheres by piston work: but a sudden stroke with this action would soon destroy the apparatus. By suddenly checking the escape of the water in one of the small pressure engines which acted by a power of upwards of one hundred atmospheres, I found that the cylinder, which was one inch diameter and half an inch thick in metal, was soon hammered out so as to exhibit external marks of the violence, and no longer to fit its piston.

The author's estimate of its power defective.

In order to form a more correct notion of the actual effect of the author's engines compared with other hydraulic machines, it may be observed that the author seems to have supposed, in p. 102, that the effect of an engine falls short of the measure of power applied to it only because of friction and impediments. I have inserted the words *generate velocity* between brackets in the text, because the case in question relates to preponderance and not equilibrium. This indispensable deduction from the power in the best and simplest engines is at least one-fourth, in order to work at a proper rate. The author greatly undervalues the engines which raise water by wheel work; and by referring to the machine at Marly he has taken perhaps the least productive which could have been any where found. (See the description, with large plates, in Desaguliers' Lectures.) As an under-shot wheel engine, it has only half the power of an over-shot; and from the prodigious weight and complication of its parts, it is not in the least adapted to be taken as the representation of works of the kind.

He underrates other hydraulic engines.

The water ram has not more power than other engines,

Though the water ram is deserving of notice for its simplicity and effect in many situations, some of which the inventor has mentioned, it does not from his facts appear to have more power than other engines. This statement, of between three-fourths and one-half of the power (page 102) being obtained in the effect, is very much to the credit of the engine, as it comes near the water pressure engine of Trevithick (Phil. Jour. v. i. p. 161.) and other good machines which are not loaded with needless parts. From general reasoning, I should not have expected so much.

—as it may raise three-fourths of what falls.

The

The water ram of M. Turquet, taken by the medium rate of estimation (namely, according to Desaguliers*) is equal to one-third part of a man, or one-fifteenth part of an horse.

The two engines he made are on a very small scale; viz. one-third and one-eighth of a man.

The engine at the author's house works at the rate of one-eighth of a man's power; so that a man would require three hours to raise as much water as the engine raises in 24 hours.

We must therefore consider these engines as models or small works capable of being multiplied: but we have not yet any practical results on a large scale.

Larger exhibitions desirable.

The water ram appears to be a very ingenious contrivance, but it is by no means any new principle or extraordinary effect in hydraulics, as some writers have pretended.

IV.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Effects of Heat. By Sir JAMES HALL, *Bart. F. R. S. Edinburgh.*

(Continued from page 22.)

SECT. VI.—*Experiments made in Platina,—with Spar,—with Shells,—and with Carbonate of Lime of undoubted purity.*

SINCE I had the honour of laying before this Society a short sketch of the foregoing experiments, on the 30th of August last (1804), many chemists and mineralogists of eminence have favoured me with some observations on the subject, and have suggested doubts which I am anxious to remove. It has been suggested, that the fusibility of the carbonates may have been the consequence of a mixture of other substances, either originally existing in the natural carbonate, or added to it by the contact of the porcelain tube.

Suspicion that the fusibility of the carbonates arose from impurity.

With regard to the first of these surmises, I beg leave to observe, that, granting this cause of fusion to have been the real one, a material point, perhaps all that is strictly necessary

If so this would not affect the inferences.

* On men and horse powers, as computed by engineers. See Philos. Journal, ix. pp. 215,—217.

in order to maintain this part of the Huttonian Theory, was nevertheless gained. For granting that our carbonates were impure, and that their impurity rendered them fusible, still the same is true of almost every natural carbonate; so that our experiments were, in that respect, conformable to nature. And as to the other surmise, it has been shewn by comparing together a varied series of experiments, that the mutual action between the lime and the porcelain was occasioned entirely by the presence of the carbonic acid, since, when it was absent, no action of this kind took place. The fusion of our carbonates cannot, therefore, be ascribed to the porcelain.

Experiments projected to determine this point.

Being convinced, however, by many observations, that the fusibility of the carbonate did not depend upon impurity, I have exerted myself to remove, by fresh experiments, every doubt that has arisen on the subject. In order to guard against natural impurities, I have applied to such of my friends as have turned their attention to chemical analysis, (a branch of the science to which I have never attended, (to furnish me with carbonate of lime of undoubted purity). To obviate the contamination arising from the contact of the porcelain tubes, I determined to confine the subject of experiment in some substance which had no disposition to unite with the carbonate. I first tried charcoal, but found it very troublesome, owing to its irregular absorption of water and air.

Pure carbonate enclosed in platina rolled up.

I then turned my thoughts to the construction of tubes or cups of platina for that purpose. Being unable readily to procure proper solid vessels of this substance, I made use of thin laminated plates, formed into cups. My first method was to fold the plate exactly as we do blotting-paper to form a filter (Fig. 26. See the quarto Plate marked 3, in vol. xiii.); this produced a cup capable of holding the thinest liquid; and being covered with a lid, formed of a similar thin plate, bent at the edges, so as to overlap considerably (Fig. 28.), the carbonate it contained was secured on all sides from the contact of the porcelain tube within which it was placed. Another convenient device likewise occurred: I wrapt a piece of the plate of platina round a cylinder, so as to form a tube, each end of which was closed by a cover like that just described (Fig. 27. and 29.). In figure 26. and 27. these cups are represented upon a large scale, and in 28. and 29. nearly

nearly of their actual size). This last construction had the advantage of containing eight or nine grains of carbonate, whereas the other would only hold about a grain and a half. On the other hand, it was not fit to retain a thin liquid; but in most cases, that circumstance was of no consequence; and I foresaw that the carbonate could not thus escape without proving the main point under consideration, namely, their fusion.

The rest of the apparatus was arranged in all respects as formerly described, the same precautions being taken to defend the platina vessel as had been used with the inner tubes of porcelain.

In this manner I have made a number of experiments during this spring and summer, the result of which is highly satisfactory. They prove, in the first place, the propriety of the observations which led to this trial, by shewing, that the pure carbonate, thus defended from any contamination, is decidedly more refractory than chalk; since in many experiments, the chalk has been reduced to a state of marble, while the pure carbonate, confined in the platina vessel, has been but very feebly acted upon, having only acquired the induration of a sandstone.

The results proved that the pure carbonate is more refractory than chalk,

In other experiments, however, I have been more successful, having obtained some results, worthy, I think of the attention of this Society, and which I shall now submit to their inspection. The specimens are all inclosed, for fafety, in glass tubes, and supported on little stands of wax, (Fig. 31. 32. 33). The specimens have, in general, been removed from the cup or tube of platina in which they were formed, these devices having the advantage of securing both the vessel and its contents, by enabling us to unwrap the folds without violence; whereas, in a solid cup or tube, it would have been difficult, after the experiment, to avoid the destruction either of the vessel or its contents, or both.

—but the results were interesting.

April 16, 1805.—An experiment was made with pure calcareous spar from St. Gothard, remarkably transparent, and having a strong double refraction. A temperature of 40° was applied; but owing to some accident, the weight was not known. The conical cup came out clean and entire, filled not quite to the brim with a yellowish-grey substance,

Pure calcareous spar underwent decided fusion.

having a shining surface, with longitudinal streaks, as we sometimes see on glass. This surface was here and there interrupted by little white tufts or protuberances, disposed irregularly. On the ledge of the cup, formed by the ends of the folded platina, were several globular drops like minute pearls, visible to the naked eye, the number of which amounted to sixteen. These seem to have been formed by the entire fusion of what carbonate happened to lie on the ledge, or had been entangled amongst the extremities of the folds, drawing itself together, and uniting in drops; as we see when any substance melts under the blowpipe. This result is preserved entire, without deranging the tube. I am sorry to find that it has begun to fall to decay, in consequence, no doubt, of too great a loss of its carbonic acid. But the globules do not seem as yet to have suffered any injury.

Similar experiment.

April 25.—The same spar was used, with two grains of water, and a heat of 35°. I have reason to suspect, however, that, in this and several other experiments made at this time, the metal into which the cradle was plunged, on first introduction into the barrel, had been too hot, so as to drive off the water. There was a loss of 6.4 *per cent.* The result lay in the cup without any appearance of frothing or swelling. The surface was of a clean white, but rough, having in one corner a space shining like glass. The cup being unwrapt, the substance was obtained sound and entire: where it had moulded itself on the platina, it had a small degree of lustre, with the irregular semitransparency of saline marble: when broken, it preserved that character more completely than in any result hitherto obtained; the fracture being very irregular and angular, and shining with facettes in various directions. I much regret that this beautiful specimen no longer exists, having crumbled entirely to pieces, notwithstanding all the care I took to enclose it with glass and wax.

Carbonate of lime purified by art was rendered crystalline and semitransparent by heat of 32°.

April 26.—An experiment was made with some carbonate of lime, purified by my friend Sir George Mackenzie. Two grains of water were introduced, but were lost, I suspect, as in the last case. The heat applied was 32°. The loss of weight was 10.6 *per cent.* Yet though made but one day after the last-mentioned specimen, it remains as fresh and entire as at first and promises to continue unchanged. The external

surface,

surface, as seen on removing the lid of the conical cup, was found to shine all over like glass, except round the edges, which were fringed with a series of white and rough sphericles, one set of which advanced, at one spot, near to the centre. The shining surface was composed of planes, which formed obtuse angles together, and had their surface striated; the striæ bearing every appearance of a crystalline arrangement. When freed from the cup, as before, the substance moulded on the platina was found to have assumed a fine pearly surface. Some large air bubbles appeared, which had adhered to the cup, and were laid open by its removal, whose internal surface had a beautiful lustre, and was full of striæ like the outward surface. The mass is remarkable for semi-transparency, as seen particularly where the air-bubbles diminish its thickness: a small part of the mass being broken at one end, shews an internal saline structure.

A shell partly fused with little change of figure at 30°.

April 29.—A cup of platina was filled with several large pieces of a periwinkle* shell, the sharp point of the spiral being made to stand upright in the cup, (fig. 30.). A heat of 30° was applied, and no water was introduced. The carbonate lost no less than 16 *per cent.* The shell, particularly the sharp end of the periwinkle, retained its original shape in a great measure, so as to be quite discernible; but the whole was glazed over with a truly vitreous lustre. This glaze covered, at one place, a fragment of the shell which had been originally loose, and had welded the two together. All the angles are rounded by this vitrifaction; the space between the entire shell and the fragment being filled, and the angles of their meeting rounded, with this shining substance. The colour is a pale blue, contrasted, in the same little glass, with a natural piece of periwinkle, which is of a reddish-yellow. One of the fragments had adhered to the lid, and had been converted into a complete drop, of the size of a mustard-seed. It is fixed on the wax (at *b*), along with the other specimens of the experiment (fig. 32.). This result shews, as yet, no sign of decay, notwithstanding so great a loss of weight.

Repetition of the experiment at 34°. fusion.

The last experiment repeated on the same day, and prepared in the same manner, with large fragments of shell, and the point of the periwinkle standing up in the cup. A heat

* Turbo terebra, *Lin*.

heat of 34° was applied; a loss took place of 13° *per cent.* All the original form had disappeared, the carbonate lying in the cup as a complete liquid, with a concave surface, which did not shine, but was studded all over with the white sphericles or tufts, like those seen in the former results, without any space between them. When detached from the cup, the surface moulded on the platina, was white and pearly, with a slight gloss. The mass was quite solid; no vestige whatever appearing, of the original form of the fagments, (fig. 33.). A small piece, broken off near the apex of the cone, shewed the internal structure to be quite saline. In the act of arranging the specimen on its stand, another piece came off in a new direction, which presented to view the most perfect crystalline arrangement: the shining plane extended across the whole specimen, and was more than the tenth of an inch in all directions. This fracture, likewise, shewed the entire internal solidity of the mass. Unfortunately, this specimen has suffered much by the same decay to which all of them are subject which have lost any considerable weight. The part next the outward surface alone remains entire. I have never been able to explain, in a satisfactory manner, this difference of durability; the last-mentioned result having lost more in proportion to its weight than this.

Artificial pure carbonate of lime from Mr. Hatchett. It resisted the heat very much;

About the beginning of June, I received from Mr. Hatchett some pure carbonate of lime, which he was so good as to prepare, with a view to my experiments; and I have been constantly employed with it till within these few days.

My first experiments with this substance were peculiarly unfortunate, and it seemed to be less easily acted upon than any substance of the kind I had tried. Its extreme purity, no doubt, contributed much to this, though another circumstance had likewise had some effect. The powder, owing to a crystallization which had taken place on its precipitation, was very coarse, and little susceptible of close ramming; the particles, therefore, had less advantage than when a fine powder is used, in acting upon each other, and I did not choose to run any risk of contamination, by reducing the substance to a finer powder. Whatever be the cause, it is certain, that in many experiments in which the chalk was changed to marble, this substance remained in a loose and brittle

—was changed into marble,

brittle state, though consisting generally of clear and shining particles. I at last, however, succeeded in obtaining some very good results with this carbonate.

In an experiment made with it on the 18th of June, in a strong heat, I obtained a very firm mass with a saline fracture, moulded in several places on the platina, which was now used in the cylindrical form. On the 23d, in a similar experiment, the barrel failed, and the subject of experiment was found in an entire state of froth, proving its former fluidity.

and in another experiment fused.

On the 25th, in a similar experiment, a heat of 64° was applied without any water within the barrel. The platina tube, (having been contaminated in a former experiment with some fusible metal), melted, and the carbonate retaining its cylindrical shape, had fallen through it, so as to touch the piece of porcelain which had been placed next to the platina tube. At the point of contact, the two had run together, as a hot iron runs when touched by sulphur. The carbonate itself was very transparent, resembling a piece of snow in the act of melting.

Similar experiment.

On the 26th of June, I made an experiment with this carbonate, which afforded a beautiful result. One grain of water was introduced with great care; yet there was a loss of 6.5 *per cent.*, and the result has fallen to decay. The pyrometer indicated 43°. On the outside of the platina cylinder, and on one of the lids, were seen a set of globules, like pearls, as once before obtained, denoting perfect fusion. When the upper lid was removed, the substance was found to have sunk almost out of sight, and had assumed a form not easily described. (I have endeavoured to represent it in fig. 31. by an ideal section of the platina tube and its contents, made through the axis of the cylinder). The powder, first shrinking upon itself in the act of agglutination, had formed a cylindrical rod, a remnant of which *(a b c)* stood up in the middle of the tube. By the continued action of heat, the summit of the rod (at *a*) had been rounded in fusion, and the mass being now softened, had sunk by its weight, and spread below, so as to mould itself in the tube, and fill its lower part completely *(d f g e)*. At the same time, the viscid fluid adhering to the sides (at *e* and *d*), while the middle part

Another experiment with perfect fusion.

part was sinking, had been in part left behind, and in part drawn out into a thin but tapering shape, united by a curved surface (at *b* and *c*) to the middle rod. When the platina tube was unwrapt, the thin edges (at *e* and *d*) were preserved all round, and in a state of beautiful semitransparency. (I have attempted to represent the entire specimen, as it stood on its cone of wax, in fig. 34.). The carbonate, where moulded on the platina, had a clean pearly whiteness, with a saline appearance externally, and in the sun, shone with facettes. Its surface was interrupted by a few scattered air-bubbles, which had lain against the tube. The intervening substance was unusually compact and hard under the knife. The whole surface (*e b a c d*, fig. 31.), and the inside of the air-bubbles, had a vitreous lustre. Thus, every thing denoted a state of viscid fluidity, like that of honey.

These last experiments seem to obviate every doubt that remained with respect to the fusibility of the purest carbonate, without the assistance of any foreign substance.

VII.

Force of the compression discussed.

Measurement of the Force required to constrain the Carbonic Acid.—Apparatus with the Muzzle of the Barrel upwards, and the weight acting by a long Lever.—Apparatus with the Muzzle downwards.—Apparatus with Weight acting directly on the barrel.—Comparison of various results.

In order to determine, within certain limits at least, what force had been exerted in the foregoing experiments, and what was necessary to ensure their success, I made a number of experiments, in a mode nearly allied to that followed by Count Rumford, in measuring the explosive force of gunpowder.

The apparatus consisted of a barrel of iron, into which the materials were put, and a cover was applied on the mouth and kept down by a loaded lever,

I began to use the following simple apparatus in June 1803. I took one of the barrels, made as above described, for the purpose of compression, having a bore of 0.75 of an inch*, and dressed its muzzle to a sharp edge. To this barrel was firmly screwed a collar of iron (*a a*, fig. 36. See quarto Plate IV. in this volume.) placed at a distance of about three inches from the muzzle, having two strong bars (*b b*) projecting

* This was the size of barrel used in all the following experiments, where the fact is not otherwise expressed.

ing at right angles to the barrel, and dressed square. The barrel, thus prepared, was introduced, with its breech downwards, into the vertical muffle (fig. 35.); its length being so adjusted, that its breech should be placed in the strongest heat; the two projecting bars above described, resting on two other bars (*c c*, fig. 35.) laid upon the furnace to receive them; one upon each side of the muffle. Into the barrel, so placed, was introduced a cradle, containing carbonate, with all the arrangements formerly mentioned; the rod connected with it being of such length, as just to lie within the muzzle of the barrel. The liquid metal was then poured in till it filled the barrel, and stood at the muzzle with a convex surface; a cylinder of iron, of about an inch in diameter, and half an inch thick, was laid on the muzzle (fig. 35. and 37.), and to it a compressing weight was instantly applied. This was first done by the pressure of a bar of iron (*d e*, fig. 35.), three feet in length, introduced loosely into a hole (*d*), made for the purpose in the wall against which the furnace stood; the distance between this hole and the barrel being one foot. A weight was then suspended at the extremity of the bar (*e*), and thus a compressing force was applied, equal to three times that weight. In the course of practice, a cylinder of lead was substituted for that of iron, and a piece of leather was placed between it and the muzzle of the barrel, which last being dressed to a pretty sharp edge, made an impression in the lead: to assist this effect, one smart blow of a hammer was struck upon the bar, directly over the barrel, as soon as the weight had been hung on.

—the fusible metal was kept fluid all the time.

It was essential, in this mode of operation, that the whole of the metal should continue in a liquid state during the action of heat; but when I was satisfied as to its intensity and duration, I congealed the metal, either by extinguishing the furnace entirely, or by pouring water on the barrel. As soon as the heat began to act, drops of metal were seen to force themselves between the barrel and the leather, following each other with more or less rapidity, according to circumstances. In some experiments, there was little exudation; but few of them were entirely free from it. To save the metal thus extruded, I placed a black-lead crucible, having its bottom perforated, round the barrel, and luted close to it, (fig. 37.);

some sand being laid in this crucible, the metal was collected on its surface. On some occasions, a sound of ebullition was heard during the action of heat; but this was a certain sign of failure.

Results.

The results of the most important of these experiments, have been reduced to a common standard in the second table placed in the Appendix; to which reference is made by the following numbers.

Experiment under the pressure of about 600lb. on the round inch, or about 50 atmospheres.

No. 1.—On the 16th of June 1803, I made an experiment with these arrangements. I had tried to use a weight of 30lb. producing a pressure of 90 lb., but I found this not sufficient. I then hung on a weight of 1 cwt., or 112 lb.; by which a compressing force was applied of 3 cwt. or 336 lb. Very little metal was seen to escape, and no sound of ebullition was heard. The chalk in the body of the large tube was reduced to quicklime; but what lay in the inner tube was pretty firm, and effervesced to the last. One or two facettes, of good appearance, were likewise found. The contents of the small tube had lost but 2.6 *per cent.*; but there was a small visible intrusion of metal, and the result, by its appearance, indicated a greater loss. I considered this, however, as one point gained; that being the first tolerable compression accomplished by a determinate force. The pyrometer indicated 22°.

The experiment was repeated the same day, when a still smaller quantity of metal escaped at the muzzle; but the barrel had given way below, in the manner of those that have yielded for want of sufficient air. Even this result was satisfactory, by shewing that a mechanical power, capable of forcing some of the barrels, could now be commanded. The carbonate in the little tube had lost 20 *per cent.*; but part of it was in a hard and firm state, effervescing to the last.

Another experiment.

No. 2.—On the 21st June, I made an experiment with another barrel, with the same circumstances. I had left an empty space in the large tube, and had intended to introduce its muzzle downwards, meaning that space to answer as an air-tube; but it was inverted by mistake, and the tube entering with its muzzle upwards, the empty space had of course filled with metal, and thus the experiment was made without any included air. There was no pyrometer used; but the heat was guessed to be about 25° where the subject of experiment lay.

day. The barrel, when opened, was found full of metal, and the cradle being laid flat on the table, a considerable quantity of metal ran from it, which had undoubtedly been lodged in the vacuity of the large tube. When cold, I found that vacuity still empty, with a plating of metal. The tube was very clean to appearance, and, when shaken, its contents were heard to rattle. Above the little tube, and the cylinder of chalk, I had put some borax and sand, with a little pure borax in the middle, and chalk over it. The metal had not penetrated beyond the borax and sand, by a good fortune peculiar to this experiment; the intrusion of metal in this mode of execution, being extremely troublesome. The button of chalk, was found in a state of clean white carbonate, and pretty hard, but without transparency. The little tube was perfectly clean. Its weight with its contents, seemed to have suffered no change from what it had been when first introduced. Attending, however, to the balance with scrupulous nicety, a small preponderance did appear on the side of the weight. This was done away by the addition of the hundredth of a grain to the scale in which the carbonate lay, and an addition of another hundredth produced in it a decided preponderance. Perhaps, had the tube, before its introduction, been examined with the same care, as great a difference might have been detected; and it seems as if there had been no loss, at least not more than one-hundredth of a grain, which on 10.95 grains, amount to 0.0912, say 0.1 *per cent.* The carbonate was loose in the little tube, and fell out by shaking. It had a yellow colour, and compact appearance, with a stony hardness under the knife, and a stony fracture; but with very slight facettes, and little or no transparency. In some parts of the specimen, a whitish colour seemed to indicate partial calcination. On examining the fracture, I perceived, with the magnifier, a small globule of metal, not visible to the naked eye, quite insulated and single. Possibly the substance may have contained others of the same sort, which may have compensated for a small loss, but there could not be many such, from the general clean appearance of the whole. In the fracture, I saw here and there small round holes, seeming, though imperfectly, to indicate a beginning of ebullition.

I made a number of experiments in the same manner, that is Method of operating with the mouth of the

barrel downwards.

is to say, with the muzzle of the barrel upwards, in some of which I obtained very satisfactory results; but it was only by chance that the substance escaped the contamination of the fusible metal; which induced me to think of another mode of applying the compressing weight with the muzzle of the barrel downwards, by which I expected to repeat, with a determinate weight, all the experiments formerly made in barrels closed by congealed metal; and that, by making use of an air-tube, the air, rising to the breech, would secure the contents of the tube from any contamination. In this view, the barrel was introduced from below into the muffle with its breech upwards, and retained in that position by means of a hook fixed to the furnace, till the collar was made to press up against the grate, by an iron lever, loaded with a weight, and resting on a support placed in front. In some experiments made in this way, the result was obtained very clean, as had been expected; but the force had been too feeble, and when it was increased, the furnace yielded upwards by the mechanical strain.

Description of the apparatus.

I found it therefore necessary to use a frame of iron, (as in fig. 38.; the frame being represented separately in fig. 39.), by which the brick-work was relieved from the mechanical strain. This frame consisted of two bars (*a b* and *f e*, figs. 38. and 39. quarto Plate III.), fixed into the wall, (at *a* and *f*.) passing horizontally under the furnace, one on each side of the muffle, turning downwards at the front, (in *b* and *e*), and meeting at the ground, with a flat bar (*c d*) uniting the whole. In this manner, a kind of stirrup (*b c d e*) was formed in front of the furnace, upon the cross bar (*c d*) of which a block of wood (*h h*, fig. 38.), was placed, supporting an edge of iron, upon which the lever rested; the working end of the lever (*g*) acting upwards. A strain was exerted, by means of the barrel and its collar, against the horizontal bars, (*a b* and *f e*), which was effectually resisted by the wall (at *a* and *f*) at one end of these bars, and by the upright bars (*c b* and *d e*) at the other end. In this manner the whole strain was sustained by the frame, and the furnace stood without injury.

The iron bar, at its working end, was formed into the shape of a cup, (at *g*), and half filled with lead, the smooth surface of which, was applied to the muzzle of the barrel.

The

The lever, too, was lengthened, by joining to the bar of iron, a beam of wood, making the whole ten feet in length. In this manner, a pressure upwards was applied to the barrel, equal to the weight of 10 cwt.

In the former method, in which the barrel stood with its muzzle upwards, the weight was applied while the metal was liquid. In this case, it was necessary to let it previously congeal, otherwise the contents would have run out in placing the barrel in the muffle, and to allow the liquefaction essential to these trials, to be produced by the propagation of heat from the muffle downwards. This method required, therefore, in every case, the use of an air-tube; for without it, the heat acting upon the breach, while the metal at the muzzle was still cold, would infallibly have destroyed the barrel. A great number of these experiments failed, with very considerable waste of the fusible metal, which, on these occasions was nearly all lost. But a few of them succeeded, and afforded very satisfactory results, which I shall now mention. Observations on this mode of experiment.

In November 1803, some good experiments were made in this way, all with a bore of 0.75, and a pressure of 10 cwt. Successful operations.

No. 3.—On the 19th, a good limestone was obtained in an experiment made in a temperature of 21°, with a loss of only 1.4 *per cent.*

No. 4.—On the 22, in a similar experiment, there was little exudation by the muzzle. The pyrometer gave 31°. The carbonate was in a porous, and almost frothy state.

No. 5.—In a second experiment, made the same day, the heat rose to 37° or 41°. The substance bore strong marks of fusion, the upper part having spread on the little tube: the whole was very much shrunk, and run against one side. The mass sparkling and white, and in a very good state.

No. 6.—On the 25th, an experiment was made with chalk, and some fragments of snail shell, with about half a grain of water. The heat had risen to near 51° or 49°. The barrel had been held tight by the beam, but was rent and a little swelled at the breech. The rent was wide, and such as has always appeared in the strongest barrels when they failed. The carbonate was quite calcined, it had boiled over the little tube, and was entirely in a frothy state, with large and distinctly Failure of one of the barrels.

tinctly rounded air-holes. The fragments of shell which had occupied the upper part of the little tube, had lost every trace of their original shape in the act of ebullition and fusion.

The compressing weight lifted with explosion.

No. 7.—On the 26th a similar experiment was made, in which the barrel was thrown open, in spite of this powerful compressing force, with a report like that of a gun, (as I was told, not having been present), and the bar was found in a state of strong vibration. The carbonate was calcined, and somewhat frothy, the heart of one piece of chalk used was in a state of saline marble.

Operations without an included air tube.

It now occurred to me to work with a compressing force, and no air-tube, trusting, as happened accidentally in one case, that the expansion of the liquid would clear itself by gentle exudation, without injury to the carbonate. In this mode, it was necessary, for reasons lately stated, to place the muzzle upwards. Various trials made thus, at this time, afforded no remarkable results. But I resumed the method, with the following alteration in the application of the weight, on the 27th of April 1804.

A direct compressing weight used and the lever rejected.

I conceived that some inconvenience might arise from the mode of employing the weight in the former experiments. In them it had been applied at the end of the bar, and its effect propagated along it, so as to press against the barrel at its other extremity. It occurred to me, that the propagation of motion in this way, requiring some sensible time, a considerable quantity of carbonic acid might escape by a sudden eruption, before that propagation had taken effect. I therefore thought, that more effectual work might be done, by placing a heavy mass, (fig. 40.), so as to act directly and simply upon the muzzle of the barrel; this mass being guided and commanded by means of a powerful lever, *(a b)*. For this purpose, I procure an iron roller, weighing 3 cwt. 7lb., and suspended it over the furnace, to the end of a beam of wood, resting on a support near the furnace, with a long arm guided by a rope *(c c)* and pulley *(d)*, by which the weight could be raised or let down at pleasure.

With this apparatus I made some tolerable experiments; but I found the weight too light to afford certain and steady results of the best quality. I therefore procured at the foundry a large mass of iron *(f)*, intended, I believe, for driving piles, and

and which, after allowing for the counterpoise of the beam, gave a direct pressure of 8.1 cwt; and I could, at pleasure, diminish the compressing force, by placing a bucket (*e*) at the extremity of the lever, into which I introduced weights, whose effect on the ultimate great mass, was known by trial. Many barrels failed in these trials: at last, I obtained one of small bore, inch 0.54, which gave two good results on the 22d of June 1804.

Trial of the least compressing force for effectual confinement of the carbonate in a fused heat. It was about 260 lbs. on the round inch, or near 22 atmospheres.

No. 8.—Wishing to ascertain the least compressing force by which the carbonate could be effectually constrained in melting heats, I first observed every-thing standing firm in a heat of above 20°; I then gradually threw weights into the bucket, till the compressing force was reduced to 2 cwt. Till then, things continued steady; but, on the pressure being still further diminished, metal began to ooze out at the muzzle, with increasing rapidity. When the pressure was reduced to $1\frac{1}{2}$ cwt. air rushed out with a hissing noise. I then stopped the experiment, by pouring water on the barrel. The piece of chalk had lost 12 *per cent*. It was white and soft on the outside, but firm and good in the heart.

Experiment with water included.

No. 9.—An experiment was made with chalk, in a little tube; to this, one grain of water was added. I had intended to work with 4 cwt. only; but the barrel was no sooner placed, than an exudation of metal began at the muzzle, owing, doubtless, to the elasticity of the water. I immediately increased the pressure to 8.1 cwt. by removing the weight from the bucket, when the exudation instantly ceased. I continued the fire for three quarters of an hour, during which time no exudation happened; then all came out remarkably clean, with scarcely any contamination of metal. The loss amounted to 2.58 *per cent*. The substance was tolerably indurated, but had not acquired the character of a complete stone.

In these two last experiments, the bore being small, a pyrometer could not be admitted.

On the 5th of July 1804, I made three very satisfactory experiments of this kind, in a barrel with the large bore of 0.75 of an inch.

Experiments with the least pressures,

No. 10.—was made with a compressing force of only 3 cwt. A small eruption at the muzzle being observed, water was thrown

thrown on the barrel: the pyrometer gave 21°: the chalk was in a firm state of limestone.

No. 11.—with 4 cwt. The barrel stood without any eruption or exudation, till the heat rose to 25°. There was a loss of 3.6 *per cent.*: the result was superior, in hardness and transparency, to the last, having somewhat of a saline fracture.

No. 12.—with 5 cwt. The result, with a loss of 2.4 *per cent.*, was of a quality superior to any of those lately obtained.

Deductions of the pressures for forming limestone, marble, and fused carbonate expressed in atmospheres and in depths of the sea.

These experiments appear to answer the end proposed, of ascertaining the least pressure, and lowest heat, in which limestone can be formed. The results, with various barrels of different sizes, agree tolerably, and tend to confirm each other. The table shews, when we compare numbers 1, 2, 8, 10, 11, 12, That a pressure of 52 atmospheres, or 1700 feet of sea, is capable of forming a limestone in a proper heat. That under 86 atmospheres, answering nearly to 3000 feet, or about half a mile, a complete marble may be formed: and lastly, that with a pressure of 173 atmospheres, or 5700 feet, that is, little more than one mile of sea, the carbonate of lime is made to undergo complete fusion, and to act powerfully on other earths.

(To be continued.)

V.

Investigation of the Temperature at which Water is of greatest Density; from the Experiments of Dr. Hope, *on the Contraction of Water by Heat at low Temperatures. By* J. Dalton.

(*Concluded from* vol. iii. p. 380.)

To Mr. NICHOLSON.

SIR,

Farther consideration of Dr. Hope's experiments.

In my last letter I apprehend it has been shewn that the results of Dr. Hope's first experiment demonstrate that water is densest at or near the 360°. I now proceed to shew that the remaining five experiments, as far as they apply, tend to establish the same conclusion.

The

The second experiment is by far the least satisfactory; indeed it may be shewn that the results are inconsistent with each other; this arises from the difficulty, not to say impossibility, of keeping up a steady ice-cold temperature in a jar containing another with comparatively warm water in it. When we consider that the water in the jar, the air, and the table are incessantly pouring heat into the ice-cold water, and that the warmer water *descends* by the exterior side of the inner jar, and by the interior side of the outer; also that the ice, which is to regulate the temperature, is every moment swimming on the top of the water when not agitated, we must be in doubt what was the *true mean* temperature of the water at the bottom of the jar during the 80 minutes which this experiment continued. From the circumstances, I judge that if the agitation were discontinued 10 minutes, the temperature at bottom would rise from 32 to 36°; however this may have been, we cannot suppose that it could be kept uniformly at 32° by "repeated cautious agitation;" whilst the temperature at top would be constantly at or near 32°. Hence the uncertainty of any conclusion derived from this experiment. If we take the experiment at the 46th minute, when the two thermometers were at 40°, and exhibit the succeeding temperatures with their differences, we shall immediately perceive an irregularity unaccountable on the supposition of uniform exterior temperature of 32°.

The second experiment objected to on various accounts.

Minutes,	Top	Dif.	Bot.	Dif.
46	40		40	
		4		0
52	36		40	
		1+		1
58	35		39	
		1–		2
65	34		37	
		0		1
75	34		36	
		0		2
103	34		34	

In the first interval, which commences with the change of the current from down to up, we observe the top loses 4°, and the bottom none. The bottom losing nothing is unaccountable, when the ascending force was a minimum, except on the supposition that the cooling liquid at bottom during this interval was 38 or 40° instead of 32°. The third interval gives 2° descent at bottom, and less than 1° at top; this cannot be the effect of an *ascending* current; and still less can the changes in the fourth and fifth intervals be ascribed to the same cause.

By frequent trials with a jar of 8 inches deep, and 2¼ diameter, containing water of 40°, and plunged into an ice-cold mixture, to the same level, in a jar of 7 inches diameter, I have found the thermometer at bottom always descends to 38° before the top one; this last generally passes the other about 37°, and even after remains lowest: after being 10 minutes at rest the top thermometer is about 35°, the bottom about 37°, and the water at the bottom of the jar at 35°, the air being 40°.

Examination of Dr. Hope's third experiments.

Dr. Hope's third experiment is an instructive one, and the conclusion he derives from it is admitted; namely, that water of 32° is not specifically heavier than water of 40°. But whether are we to infer from it that water of 40° is *equal* to water of 32° in density, or that it is *superior*? I think the former. Whenever a column of water of 32° is situated on another warmer column of the same density, (whether it be 40 or 48°) there can be no current generated from the mutual interchange of heat of the two columns, neither immediately nor remotely. The connecting film, or stratum, will indeed instantly assume the intermediate temperature of greatest density (of 36 or 40°), and the contiguous strata above and below will gradually shade off into warmer and colder water; hence the horizontal strata will be of different specific gravities, but the *perpendicular* columns of particles will all be of the same gravity, and therefore no motion ensue. Now does not the experiment testify that this state takes place when the temperature is at 40°, and not at 48°? Hence we must infer from it that water of 40° is of the same specific

gravity

gravity as water of 32°, and, consequently, that the point of greatest density is at 36°.

Inference that water is densest at 36°.

Dr. Hope was aware of the plausibility, if not of the force, of this reasoning, and therefore judiciously admits that this experiment does not decide the question; this is supposed to be done by the 4th experiment, which exhibits a tall jar of water of 40°, placed some inches deep in a pan containing ice and salt. Here we have water of 40°, and a cold mixture of 0°, with a partition of glass betwixt. The medium temperature of that part of the glass jar immersed, may be stated at 20°, just after the commencement of the experiment. The contiguous water is soon cooled to 32°, whilst the temperature of the mixture remains nearly stationary for a long time; whence the mean of the glass would soon be 16 or 18°: now it must follow that the water, in contact with the glass, was liable to be cooled *below* the freezing point, provided it was susceptible of such temperature; and all experience shews that water, in such circumstances, not only *may*, but nearly always *does*, cool several degrees below freezing, before congelation commences. If then the marginal water was cooled to 25 or 28°, whilst the central was 36 or 38°, in so tall a column, a prodigious force of ascent would be produced, sufficient to explain the phenomena, whether we suppose the point of greatest density at 40°, at 36°, or even at 32°.

The fourth experiment.

Reasoning by which the phenomena may be explained, with a great uncertainty as to the most density.

The fifth experiment of Dr. Hope is one which more especially involves a question of primary importance to the subject. That is, suppose a liquid of 32° contracts by heat to a certain temperature, and after that expands again so as to equal its former bulk by another equal augmentation of temperature; again, suppose that a tall jar of such liquid at the temperature of 32° was surrounded in the middle by a zone or belt kept constantly at a temperature superior to that at which the liquid is of greatest density; *quære* the manner in which the heat acquired ought to be distributed through the liquid?

Dr. Hope's fifth experiment.

Question how the heat was distributed through the liquid.

In answer to this it may be observed, that reasoning *a priori*, the effect ought to be different according to the periods of time, which may be divided into three; and the *first* period will be that in which the marginal liquid descends to the bottom of the vessel till it there becomes of the greatest density; the

Particular statement of the effect.

second is that in which the liquid still descends, but so as to produce regular strata from the bottom, increasing in temperature upwards to the middle, where it at last becomes of that temperature which is of the same specific gravity as the upper liquid at 32°; the *third* is that in which all the heat acquired ascends into the upper half of the vessel. Let us now see whether the 5th experiment of Dr. Hope will more favour the notion of greatest density at 36 or at 40°. In transcribing his results, I will add a column denoting the temperature at the middle of the vessel, such as I apprehend it would have been found by a thermometer. It must therefore be noticed, that the middle column is an imaginary one, and not obtained from Dr. Hope's observations.

The observations were:

	Bottom.	Middle.	Top.
At commencement,	32	32	32
In 10 minutes,	35	34	32
15	36—	35	32
20	36+	36	32
25	37	37	33
30	38	38	33
38	38+	40	33
45	39—	42	33
50	39+	44	44
55	39+	46	45
60	39+	48	48

Induction that the greatest density is at 36° and not 40°.

Now if we take 30° as the point of greatest density, the first period of time will be 20 minutes, after which, the bottom thermometer was found at 56°+, the middle probably 35 or 36°, and the top one unaffected at 32°; the second period will terminate about 38 or 40 minutes, when the bottom thermometer was 38+, the middle 40° (indicating the same specific gravity as 32°), and the top 33°, having only gained 1°; the third terminates with the experiment; during this period the bottom thermometer has gained 1°, which may easily be accounted for by the natural tendency to equalization of temperature in the lower half of the jar; the middle of the jar, being the focus of heat, may be supposed about equal to the top

top in temperature, and to have gained 8°, whilst the top has rapidly passed from 33° to 48°, an increase of 15°.—If we take 40° for the point of greatest density, the bottom never attains it during the whole experiment, whilst the middle and top gradually arrive at, and surpass it long before the conclusion.

Dr. Hope's sixth experiment with a cooling zone.

The sixth experiment will now be easily explained. A frigorific mixture was put into the vessel surrounding the middle of the tall jar, which contained water of 39½°. The thermometer was observed as under:

		Bottom.		Top.
At commencement,	—	39.5	—	39.5
In 10 minutes,	—	39 +	—	38 +
25 ——	—	39 +	—	36.5
35 ——	—	39	—	36—
55 ——	—	39	—	35
1 h.-10 ——	—	39 —	—	34 +
—— 35 ——	—	39 —	—	34 —
2 — — ——	—	39 —	—	33 +

Reasons for concluding hence that the greatest density is at 36°.

Here the first observation is sufficient of itself to decide by which theory the whole are to be explained. In 10 minutes we observed a fall of nearly half a degree at the bottom, and one of 1½° at top. How will Dr. Hope account for the descent of the bottom thermometer? Water of 36° or 37° cannot descend into water of 39.5, that of less density into greater; it must then be the effect of the propagation of cold downwards by the proper conducting power of the liquid. Granted: but if this was the case, a thermometer in the centre of the jar, should have indicated 32°; and one in the middle, between the centre and the bottom 36°, or thereabouts; for every one allows, that in the proportion of heat (or cold) along any solid body, the effect is produced *gradatim*. This conclusion, however, would ill accord with what was observed in the third experiment. Let us now try this experiment by the other point, or 36°.—The sudden cold applied to the middle of the jar would quickly reduce the contiguous liquid to 36°, and below; this gives it a force of descent by which the tempera-

ture

ture below is reduced; but as soon as it gets to 39°, the force of descent is weaker, and the quick application of cold shortly reduces the marginal temperature to 32°, or below, by which the whole current is turned upwards; a slight diminution of temperature at bottom is, notwithstanding, still perceived as long as any water is found in the jar of 36°, by its constant superintendency downwards.

Conclusion. Thus I apprehend it is made appear that those who have hitherto investigated the point of temperature at which water is most dense, from experiments of a similar nature to those above, have mistaken it; that the true point of greatest density is at 36°, and that the density at 32° and at 40°, or at any other two equal intervals above and below 36°, is nearly the same, progressively diminishing alike by the addition or abstraction of heat from the said point.

I am your's, &c.

J. DALTON.

Manchester, May 12th, 1806.

VI.

Analysis of a new Mineral found in Cornwall. By J. KIDD. *M. D. &c. &c.*

To Mr. NICHOLSON.

SIR,

Oxford, May 9th, 1806.

Analysis of a new mineral. I HAVE taken the liberty of sending you an account of the analysis of a new mineral lately met with in Cornwall; and, if you think it of sufficient consequence, shall be obliged by your giving it a place in your Journal whenever it is convenient.

Its history. The mineral in question was met with in one of the Gwennap mines, and formed an incrustation round projecting particles of a spongy pyrites intermixed with quartz; this pyrites appeared to contain a considerable proportion of cobalt, since it produced a deep blue colour when fused in a very small quantity with glass of borax: the incrustation itself was supposed by the miners to be a variety of wood-tin. Its appearance was altogether

altogether new to those most acquainted with the mineral productions of Cornwall.

Its colour both externally and internally varied from a light ash to a dark brown; fracture like that of flint, presenting sections of concentric layers; texture close and polished like that of a nut, and of a silky lustre; hardness about 8 of Kirwan; not easily broken in the mass, but its small fragments very brittle: when triturated giving out a strong hepatic odour; sp. gr. varying from 3.7 to 3.9. External characters.

Soluble in nitric and muriatic acids with effervescence, violently decomposing the former, and giving out sulphureted hydrogen gas in abundance with the latter; in both instances depositing a considerable proportion of sulphur. Solulable in acids like a metal.

Precipitable from the above acid solutions by aqua kali in a soft gelatinous form of a light cream colour, but becoming of a pale olive green at 300° Fahr.; sp. gr. of this precipitate about 4.5: the same change of colour took place at the same heat in some earthy calamine from Derbyshire, of the sp. gr. 3.6764. Precipitable by alkali,

Precipitable entirely from the acid solutions by prussiate of potash; colour of the precipitate a light French grey. —and precipitate of potash.

By the heat of an argand lamp losing about $\frac{1}{100}$ of its weight, owing to loss of water; in a low red-heat losing about $\frac{34}{100}$ of its weight; by a strong red-heat, in close vessels sublimed, in part, in the form of minute acicular crystals of the silvery appearance of similar crystals of flowers of zinc; in the strongest heat of a moderate forge, sublimed in small prismatic crytals of a brown colour, and adhering firmly to the sides of the crucible; these crystals when viewed through a microscope were in colour and lustre very like brown semitransparent blende, and were soluble in the nitric and muriatic acids with phenomena similar to those attending the solution of blende in the same acids. Its habitudes by heat.

It seems worthy of remark, that a quantity not exceeding 50 grains being reduced to powder and exposed to a moderate forge-heat in a small crucible of platina, prepared by Dr. Wollaston, enclosed in another made of earthen ware, the platina where in contact with the mineral was completely fused, and the remaining part was covered with an iridescent pellicle, and made soft and brittle throughout its substance. It gives fusibility, &c. to platina.

A simi-

Comparative experiments with zinc and blende.

A similar effect was lately produced on a platina spoon, in which some galena was exposed to the blow-pipe.

As the quantity of the mineral under examination detached at different times did not in the whole amount to above 300 grains, of which the greater part had been wasted in the foregoing experiments, an analysis of so small a quantity would not have been published, but that unfortunately another specimen could not be procured of the same mineral, : in order however, to render as satisfactory as possible the results of an analysis conducted on so small a scale, a few comparative experiments were made on zinc, and on blende, of which last substance this mineral evidently appeared to be a variety; an account of these experiments has been added, as not altogether uninteresting in themselves, but principally in confirmation of the analysis: the reagents employed in these, were prepared by Messrs. Allen and Howard; and the precipitates were washed, separated, and dried, without the use of filters, except in the case of the precipitates by prussiate of potash,

Experiments on zinc.

	grs.
Zinc, grs. 5, dissolved in nitric acid (1.47), precipitated by potash, and dried at 300 Fahr. weighed	6.40.
Zinc, grs. 20, dissolved, &c.	24.50.
Zinc, grs. 100, dissolved, &c.	125.

Sp. gr. of these precipitates was about 4.3.

According to Mr. Proust's analysis, (vide Thompson, 2d. edition, vol. 1. p. 199,) the two first of the three preceding precipitates ought to have weighed respectively 6,25, and 25: the weight of the last is correct.

Experiments on blende.

	grs.
Laminated brown blende (sp. gr. 4.0678), grs. 100, dissolved in nitric acid, precipitated by potash, and dried at 300 Fahr. weighed	75.

Sp. gr. of the precipitate 4.54.

Zinc, grs. 5, dissolved in nitric acid, precipitated by prussiate of potash, and dried at 300 Fahr. weighed	16.50.
Zinc, grs. 20, dissolved, &c.	65.
Zinc, grs. 100, dissolved, &c.	330.

The weights of the three last precipitates would bear equal proportions to the respective weights of the zinc taken, if the two first, instead of 16.50 and 65, were 16 and 66.

As

Useful application of galvanism in analysis.

As it was evident that the mineral under examination contained oxide of zinc, and as the usual mode of reducing this (particularly in minute quantities) is extremely difficult, the process of reduction was varied in the following manner. It is generally known, I believe, that by means of the galvanic apparatus, the metals may be precipitated on each other from their acid solutions without regard to the usual order of chemical affinity: the objection to the application of this process, as a means of analysis, arises from the quick re-absorption of the precipitated metal by the disengaged acid from which it had just been separated: but, as Mr. Klaproth has lately observed that the metals may be precipitated by each other from the alkaline solutions of their oxides in the order of their chemical affinity, a portion of oxide of zinc was dissolved in aqua kali, and by means of the galvanic apparatus a precipitation of metallic zinc was very readily obtained on plates of iron, copper, and platina; in this experiment it was accidentally observed, that upon withdrawing the communication with the galvanic apparatus, the recently precipitated zinc was soon re-dissolved by the aqua kali, and not only by the portion of aqua kali, employed in that experiment, but by any other portion also, or even by a solution of sub-carbonate of ammonia; however, by not interrupting the communication, the precipitate remained; and in this way, four parts out of five of metallic zinc were recovered from a solution of its oxide in aqua kali. The re-absorption of the zinc by the aqua kali at first appeared to take place without any effervescence; but upon a closer examination, minute air-bubbles were seen detaching themselves from the surface of the zinc; if these were bubbles of hydrogen gas, as probably they were, originating from the decomposition of the water, an easy explanation is afforded of the solution of the zinc.

By the foregoing experiments were obtained the means of ascertaining, both by calculation and actual reduction, the proportion of zinc in a given weight of its oxide. In order to ascertain the quantity of sulphur acidified during solution in nitric acid, the following experiment was made:

To ascertain the quantity of sulphur acidified by solution in nitric acid.

Sulphur, grs. $7\frac{2}{10}$, were boiled in nitric acid till the whole of the sulphur had disappeared: a solution of nitrate of baryt was then added till there was no longer any precipitation; the precipi-

precipitate being then washed, and dried at a low red-heat, weighed grs. 50, (sp. gr. of it was 3.7826). This experiment must be considered as very satisfactory, from its near agreement with M. Cheneoix's statement; from which it only differs in the proportion of 7.20 to 7.22, (vide Thompson, 2d edit. vol. ii. p. 20.)

Analyses of different portions of the mineral.

It remains to give an account of two or three analyses of different portions of the mineral itself; in which, though the quantity of silex is noticed, yet as its presence is merely accidental, it is excluded from the whole sum in calculating the proportions of the other substances.

I.	grs.
Quantity taken	30.
Detached particles of minutely xtallized quartz, separated during solution in nitric acid.	0.5.
Sulphur separated during solution	1.6.
Do. by calculation in sulph. of baryt, grs. 54	7.8.
Precipitate by potash, dried at 300 Fahr.	20.
	29.9.

Proportion of sulphur, about $\frac{32}{100}$.
——— of precipitate $\frac{68}{100}$.

II.	grs.
Quantity taken	13 $\frac{8}{10}$.
Detached particles of quartz	0.4.
Sulphur separated	1.2.
Do. in sulph. of baryt, grs. 20	2.9.
Precipitate by potash	9.
	13.5.

Proportion of sulphur about $\frac{31}{100}$.
——— of precipitate $\frac{66}{100}$.

III.	grs.
Quantity taken	22.
Driven off by a strong red-heat	8.
Precipitate by potash from nitric acid	13.5.
	21.5.

Proportion of sulphur in the third analysis almost $\frac{38}{100}$.
Proportion of the precipitate by potash not quite $\frac{63}{100}$.

It

It is probable, that in this instance part of the substance precipitable by the potash was driven off by the high degree of heat to which it was exposed: and this would account for the difference in the proportions of this analysis from those of the preceding.

IV.	grs.
Quantity taken	13.
Sulphur separated during solution in nitric acid......	1.5.
Ditto in sulph. of baryt, grs. 19..................	2.7.
Potash precipitate at 300 Fahr...................	9.
	13.2.

Proportion of sulphur about........ $\frac{32}{100}$.
——— of precipitate .... $\frac{69}{100}$.

In this last analysis, the precipitate by potash was afterwards thrown into distilled water, and dissolved, as far as could be, by potash; a dirty-coloured flocculent substance, which remained undissolved, was caught on a filter: this was readily dissolved by muriatic acid, and gave a deep blue colour on the addition of prussiate of potash; but the prussiate of iron thus obtained was too small in quantity to afford a satisfactory examination.

The filtered potash solution was exposed to the action of the galvanic apparatus, and at different times $5\frac{8}{10}$ grains of a metallic substance were recovered from it, which resembled zinc closely in colour, and more closely in its property of burning with a blue flame when thrown on ignited iron, and leaving a white oxide on the surface of the iron.

But $5\frac{8}{10}$ grains of metallic zinc equal $7\frac{25}{100}$ of oxide: therefore 9 grains of the precipitate having been dissolved in the potash, there remained $1\frac{75}{100}$ grains unrecovered (including, that is, the small proportion of iron which had been separated by filtration). The potash solution was now neutralized by nitric acid, and in this manner $\frac{30}{100}$ grains of the original precipitate were recovered; which being added to the $7\frac{25}{100}$ separated by the galvanic apparatus, leaves only $\frac{25}{100}$ of the original 9 grains unaccounted for.

The grains $5\frac{8}{10}$ of metallic zinc obtained in the above manner, when dissolved, in part, in diluted sulphuric acid, gave a perfectly white precipitate with prussiate of potash.

By comparing the foregoing experiments and analyses with each other, this mineral appears to consist of about

33 sulphur,
66 oxide of zinc,
—
99

with a very minute proportion of iron.

I am afraid the preceding account will be thought tedious by those who are in the habit of chemical analysis; but not having been much accustomed to this myself, it seemed the safer way to be as particular as possible in the relation of the foregoing experiments.

I am, Sir,

Your most obedient servant,

J. KIDD.

VI.

Second Essay on the Analysis of Animal Fluids. By JOHN BOSTOCK, *M. D. Liverpool.**

Method of analysing compound animal fluids.

IN my former essay I endeavoured to ascertain a definite character for the three primary animal fliuds, albumen, jelly, and mucus, and to point out tests by means of which their presence might be detected with facility and precision. I now propose to offer some observations upon the method to be employed in the analysis of those compound fluids, of which the three substances above mentioned form a principal part. I shall arrange my remarks according to the order adopted in my former paper, beginning with the consideration of the albumen.

Separation and propagation of albumen.

My first object was to discover some method by which the exact proportion of this substance might be ascertained in any fluid of which it formed a component part. The application of caloric, as appears from my former experiments, affords a very accurate test of the presence even of the smallest quantity of albumen; but I found that it was not possible, by this agent, to

* Received from the author; who has also inserted it in the Medical Journal. The former Essay is in our Journal, xi. 244.

sepa-

separate it from the water, or other substance, with which it is combined. When a solution containing $\frac{1}{30}$ of its weight of pure albumen was kept for some time at the boiling temperature, the whole fluid assumed an opake and semi-gelatinous appearance; but the water still remained so far attached to the solid matter, that it scarcely passed at all through a filter of bibulous paper: a part of it was not transmitted even after it had lain upon it for several days, and was beginning to exhibit marks of putrefaction. When albumen exists in that state of concentration in which it is found in the white of the egg, *i. e.* composing about 15 parts in the 100, it is capable, as we know, of becoming so completely concreted as to resemble a solid substance, and, if it be divided into small pieces, it may be digested in hot water, without its figure or consistency being affected.

Mere heat has little effect as to complete precipitation.

It appeared a subject of some importance, to ascertain the degree of dilution of which albumen admits without losing this property, as, by this means, some general idea might be formed of the proportion of it in any compound fluid, merely by the application of caloric, in those cases where we may not have it in our power to enter upon a more minute examination. I found that the white of the egg, after being mixed with half its weight of water, still retained the power of becoming so far coagulated, that the figure of its parts, when divided by a knife, was not altered; but that when an equal weight of water was added to the white of the egg, though it was rendered completely opake by heat, yet it still retained some part of its fluidity, so that it might be slowly poured from one vessel to another. In the former case the albumen composed somewhat less than $\frac{1}{10}$ part of the weight of the fluid, and in the second about $\frac{1}{13}$.

One-tenth of albumen in water, renders it solid if heated;—one-thirteenth does not.

I had next recourse to the oximuriate of mercury, which I had before found to be, as it were, the appropriate coagulator of albumen. I experinced, however, the same kind of difficulty in this case, as in the employment of caloric. Notwithstanding the delicacy with which the oximuriate of mercury detects the most minute portion of albumen, I found the coagulation to be so complete, that the fluid continued to retain a considerable degree of opacity, after being passed through a filter, and to be still coagulable by the application of heat, even when it indicated an excess of the oximuriate. The entire separation of the albumen seemed, however, to be attained by the union of both

Oximuriate of mercury presents the same difficulty of heat:

—but this salt aided by heat is effectual.

both these methods, *i. e.* by subjecting the fluid to the boiling temperature, after the addition of a requisite quantity of the oximuriate of mercury. That we may be assured that a sufficient quantity of the metallic salt has been employed, it is necessary that it be added a little in excess; a circumstance which may be easily ascertained, by observing whether the filtered fluid possess the power of precipitating a fresh solution of albumen.

The precipitate contains oximuriate:

The precipitate produced by the joint operation of caloric and the oximuriate of mercury is a compound of albumen and the metallic salt; so that, before we can ascertain the quantity of the former, it will be necessary to learn in what proportion they are disposed to combine with each other. But this point, simple as it may appear, is not unattended with difficulty; it is not easy to collect and detach from the filter a substance of this peculiar texture; and much nicety is requisite in the subsequent drying, so that all the moisture may be completely expelled, and yet that the substance should not experience any commencement of decomposition. Making the experiment with the requisite precautions, it appeared to me that albumen, when coagulated by the addition of the oximuriate of mercury,

—quantity.

unites itself to between $\frac{1}{3}$ and $\frac{1}{4}$ of its weight of the salt. If this estimate be confirmed by more extensive experiments, it will be easy to calculate, with tolerable accuracy, the quantity of albumen in any compound animal fluid, by employing a solution of the oximuriate of mercury of a known strength, and observing what quantity it is necessary to saturate a given quantity of the body under examination. If, for example, we find that 100 grains of the fluid require 60 grains of a solution containing $\frac{1}{20}$ of its weight of the oximuriate of mercury, it will follow, that it contains 10.5 grains of albumen.

The uncoagulable part of white of egg is mucus.

Before I leave the subject of albumen, I shall make some remarks upon the uncoagulable part of the white of the egg. I found it very generally to constitute about $\frac{1}{4}$ of the weight of the whole solid contents, as stated in my former essay. A solution of this substance, in about 100 times its weight of water, was not effected by the addition of the oximuriate of mercury, or the decoction of galls, but a single drop of the aqua lithargyri acetati threw down a copious precipitate. I gradually evaporated the fluid, and occasionally stopped the process when

it

it was nearly completed; but I did not observe any tendency towards gelatinization, or the exhibition of any crystalline appearance. I concluded, therefore, that it consisted altogether of mucus.

In the course of my experiments on albumen, particularly those made during the summer months, I have observed, that this substance is less disposed to become putrid in its natural state than when diluted with a greater proportion of water, and that a solution of the mucilaginous part, formed by washing the coagulated albumen, was still more subject to decomposition. In some instances, where I permitted a diluted solution of the albumen ovi to become putrid, I was forcibly impressed with the resemblance of its odour to that of pus; whereas the putrid mucilage discharged the usual nauseous smell.

Putrid albumen resembles pus.

With respect to the saline ingredients of the albumen ovi, they seem to exist in very minute proportion. I was never able to detect any visible indication of saline matter by the evaporation of the water in which coagulated albumen had been washed; a considerable precipitate was indeed produced by the addition of the nirate of silver; but I concluded, from its appearance, that at least the greatest part of the effect depended upon the coagulation of the animal matter, though some part of it might be due to the presence of the muriate of soda. The albumen ovi exhibits slight alkaline effects upon the appropriate test papers; and, by means of the oxalic acid, a very minute trace of lime may be detected, which probably exists in combination with the phosphoric acid. In order to ascertain the quantity of alkali, I formed a very diluted alkaline solution of a known strength, and observed how much acetous acid was necessary to neutralize a given weight of it. With the same acetous acid I neutralized a portion of the white of the egg, and, making the necessary calculations, I estimated that 100 grains of the albumen ovi contain no more than $\frac{1}{12}$ of a grain of alkali. This alkali has generally been supposed to be soda, and as this salt is more frequently present in the different parts of the animal body than potash, we may conclude, with some plausibility, that it is soda which exist in the albumen ovi. It has been supposed to exist in the pure or caustic state; but I am not aware of any method by which this circumstance can be ascertained. I added the carbonate of soda to a solution of albumen ovi, in considerably

Albumen ovi contains scarcely any saline matter: —perhaps muriate of soda: —a trace of lime; —and a thousandth part of soda.

siderably greater quantity than that indicated above, yet the addition of the sulphuric acid produced no visible effervescence. I think it must therefore remain undetermined, whether the alkali exist in the pure or carbonated state.

Concerning jelly: It is easily separated, and its quantity determined by tannin.

The method of ascertaining the exact quantity of jelly in any compound fluid is, upon the whole, more easy. Isinglass affords us the means of obtaining jelly in a state of almost perfect purity; by forming a solution of this substance, and an infusion of galls of a known strength, by adding them to each other until they are neutralized, and collecting the precipitate, we can ascertain the respective proportions necessary to produce the neutral compound. As the precipitate formed in this case subsides slowly, it is more convenient, after the mixture of the jelly and the galls, to filter the compound, and to add a little of the filtered fluid to fresh solutions of jelly and galls respectively; from observing in which of the solutions a precipitate is produced, we are enabled to determine which ingredient exists in excess, and to correct the deficiency in a subsequent experiment; this process must be repeated until the filtered fluid produces no precipitate with either of the reagents. By proceeding in this manner I am led to conclude, that the compound formed by the union of jelly and tannin consists of somewhat less than two parts of tannin to three of jelly; as we always have it in our power to ascertain the quantity of tan that we employ, we may, by an easy calculation, deduce the amount of the jelly in any fluid under examination.

The proportion of mucus not easily determined.

I have not yet been able to fall upon a method for directly determining the proportion of mucus in a compound fluid, in consequence of the facility with which goulard decomposes the different ingredients, both animal and saline, which are always to be suspected in those substances that contain mucus, even in a state the nearest approaching to purity. Muriate of soda is, I believe, always present wherever we have mucus; and the goulard, which so readily and completely precipitates the mucus, likewise decomposes the common salt. The nitro-muriate and the muriate of tin, and the nitro-muriate of gold, all cause a considerable precipitation in a solution of salvia; but the supernatant fluid remains opake, as if it still contained some animal matter; and, in consequence of the muriatic acid which enters into the composition of these salts, we are not able after-

afterwards to search for the muriate of soda, by applying the test of the nitrate of silver. The nitrate of silver itself, although it scarcely porduces any effect upon a solution of vegetable gum, when it is added to saliva, throws down a very copious precipitate, partly of a dense powder, and partly of a flocculent matter: This, I apprehend, proceeds from its acting both upon the mucus and the muriate of soda. The nitro-muriates of tin and of gold do not decompose common salt, but they precipitate albumen as well as mucus, and on this account cannot be employed. The only way of proceeding that I have been hitherto able to employ is to discover the quantity of albumen and of jelly by the methods mentioned above, and after deducting their weight from the whole of the solid contents, to consider the remainder as mucus; but here we are necessarily confounding the mucus and the salts. After this statement, I need not add that the subject still requires farther elucidation.

Indirect method.

I have attempted, in a few cases, to apply my ideas respecting the analysis of animal fluids to the actual examination of some substances, and shall now proceed to detail my experiments. I must premise that, in the two first analyses, the small quantity upon which I was obliged to operate prevented me from determining the proportion of the ingredients as accurately as I could have wished. I have nevertheless inserted them, as these fluids are not at all times to be procured.

Examination of some other animal fluids by these methods.

The first set of experiments which I performed were upon the fluid discharged, by puncturing a tumour formed on the spine in the disease which is usually called *spina bifida*.

Discharge from a tumour.

1. The fluid was colourless, slightly opake, and gelatinous, of a specific gravity, scarcely differing from that of water, and insipid.

2. It did not affect either litmus or an infusion of mallows.

3. A hundred grains of the fluid were slowly evaporated; a residuum was left of 2.2 grains only.

4. When kept for some time at the temperature of boiling water, its opacity was slightly increased, but it did not exhibit any tendency to coagulation.

5. A saturated solution of the oximuriate of mercury, when first added, produced but little effect; after some time, however, an inconsiderable precipitate was thrown down.

Examination of the liquid from a tumour.

6. Infusion of galls produced a precipitate in small quantity.

7. Aqua lithargyri acetati produced a copious dense precipitate.

8 By the addition of the nitro-muriate of tin, the fluid was rendered considerably more opake, and, as if approaching to coagulation after some time a precipitate was formed.

9. The residuum from No. 3. was partly dissolved by being digested in hot water.

10. The water from No. 9. produced a copious precipitate with nitrate of silver.

11. It also produced a perceptible precipitate with oxalic acid.

12. It also produced a slight precipitate with the infusion of galls.

13. A quantity of this fluid, being evaporated very slowly, left cubical crystals of common salt in considerable quantity.

From No. 3. we learn that 97.8 parts in 100 consist of water. From 4. and 5. we learn that it contained a little albumen. The quantity was too small to be collected and measured by weighing; but, from the visible effect produced by heat and the oximuriate of mercury, I should conceive it could not be more than $\frac{1}{200}$ of its weight. From No. 6. and 12. and by comparing 6. with 5. we learn that it contains a minute quantity of jelly. From 7. and 8. especially by comparing them together, we learn that it contains mucus. By comparing 7. and 8. and from 10. and 13. we learn that it contains the muriate of soda in considerable quantity; and from 11. that it contains a very small trace of lime. The composition of the fluid will therefore be nearly as follows:

Water	97	8	
Muriate of soda	1	0	
Albumen	0	5	
Mucus	0	5	These proportions are in some measure conjectural.
Jelly	0	2	
Lime	/		a very minute quantity.
	100	0	

Examination of the liquor pericardii.

The next fluid that I had an opportunity of examining was the *liquor pericardii*, which was obtained by opening the body of a boy

a boy who had died suddenly, in order to ascertain the cause of his death. The whole quantity collected was about half an ounce; it was nearly of the colour and appearance of the serum of the blood. Examination of the liquor pericardii.

1. A quantity of it was slowly evaporated, and a residuum was left, amounting to $\frac{1}{13}$ of the whole.

2. A quantity of the fluid was kept for some time at the heat of boiling water; it became considerably opake and gelatinous.

3. A copious precipitate was produced by the oximuriate of mercury.

4. After the fluid was saturated with the oximuriate of mercury, it produced no precipitate with the infusion of galls.

5. The nitrate of silver produced a precipitate which indicated both animal matter and the muriate of soda.

6. A quantity of the coagulated fluid, No. 2, being dried in the temperature of boiling water, was afterwards washed with boiling distilled water.

7. The water from No. 6. gave no precipitate with the oximuriate of mercury, nor with galls, but a pretty copious one with the aqua lithargyri acetati.

The small quantity of the fluid which I was able to obtain prevented me from prosecuting the analysis with more minuteness; from these experiments, however, we may form some idea of its composition. From the 1. we learn that it contains 92 of water; from No. 2. and 3. that it contains a considerable quantity of albumen, which I should estimate at somewhat more than $\frac{1}{20}$ of its weight. No. 4. and 7. show that it contained no jelly. No. 7. that it contained mucus; and No. 5. that it contained common salt, but the proportion of this latter appeared not very considerable. The constituents of the liquor pericardii will therefore be:

Water	92. 0	
Albumen	5 5	
Mucus	2 0	The proportion of these substances is somewhat conjectural.
Muriate of soda	0 5	
	100 0	

The next analysis that I attempted was that of the saliva; this fluid, in its natural state, is mixed with such variable pro- Examination of the saliva.

Examination of the saliva.

proportions of water, that it is almost impossible to fix any standard which can be considered even as the average quantity. It is, however, convenient, in observing the effects of reagents upon it, to have it in a more diluted state than it usually occurs; and I accordingly united it, by rubbing in a mortar, with a quantity of distilled water, until, by evaporation, 100 grains of the mixture were found to contain two grains of solid residuum. Upon this mixture the following experiments were performed.

1. The fluid was still opake, and there was an appearance as if some flocculent matter were suspended in it.

2. No effect seemed to be produced by exposing it to the boiling temperature.

3. When the oximuriate of mercury was added, no immediate visible effect was produced, but after some hours, a light flocculent coagulum separated and fell to the bottom, having the fluid nearly transparent.

4. A portion of the fluid, left for a few days without addition, gradually suffered a quantity of matter to separate from it, as in No. 3.; but the separation was less complete, and it was much longer in taking place.

5. A quantity of the fluid being passed through a filter of bibulous paper, was rendered perfectly transparent.

6. The oximurate of mercury being added to a quantity of No. 5. a very slight precipitate only was produced after some time.

7. The addition of the infusion of galls to No. 1. caused a precipitation of white flakes; but, after filtration, the galls produced no effect.

8. The filtered fluid, No. 5. produced a copious precipitate with the aqua lithargyri acetati.

9. It also produced a considerable precipitate with the nitro-muriate of tin.

10. And with the nitrate of silver.

11. Equal weights of the fluid, before and after filtration, were separately evaporated, and the amount of the residuum being ascertained, the quantities left were to each other nearly as 12 to 8.

12. The diluted saliva, both before and after filtration, slightly reddened a paper stained with litmus.

From

From these experiments we may draw the following conclusions: From No. 3. it would seem that the fluid contains albumen; but it appears from Nos. 1. 2. 4. 5. and 6. that the albumen is not soluble in water, but in that state in which it is found after coagulation. From this we learn that it constitutes only 0.8 of a grain in 100 grains of the fluid. From No. 7. we learn that there is no jelly; from 8. 9. and 10. that there is a quantity of mucus and muriate of soda; and, from comparing these with each other, we are led to conclude, that the last substance exists only in small quantity. The composition of the diluted saliva will therefore be nearly as follows: Examination of the saliva.

Water	98 0	
Coagulated albumen	0 8	
Mucus	1 1	The proportion of these is partly conjectural.
Salts	0 1	
	100 0	

It will, I conceive, be admitted, that the albumen in this saliva existed in coagulated state. This I consider to be decidedly proved from the effects of heat, by its gradual, spontaneous deposition, and by the ease with which it was separated by filtration. Still, however, the oximuriate of mercury and the galls showed that it was albumen. The difficulty of uniting saliva with water, and the effects of filtration, were noticed by Dr. Fordyce; * but he imagined that the whole of the animal matter was removed by the process. The saliva that I employed manifested slightly acid properties: How far this may be the case in general, I am unable to decide. Haller thinks that, in a state of perfect health, the saliva is not acid; but, at the same time, he quotes a number of authors who are of a contrary opinion. † M. Hapell de la Chenaie informs us, that the saliva of the horse is alkaline. ‡

The quantity of water contained in the saliva, as discharged from the mouth, is very various. Haller estimates it at about $\frac{4}{5}$ of the whole; but Dr. Fordyce supposes that $\frac{1}{12}$ only consists of solid matter. If we take the estimate of Haller,

* De Catarrho, p. 17.

† El. Phys. lib. xviii. sect. 2. § 10.

‡ Mem. of Med. Soc. for 1780-1, p. 325.

which

Examination of the saliva.

which is sanctioned by Fourcroy * and Thomson †, the constituents of saliva will exist in the following proportions:

Water	80 0
Coagulated albumen	8 0
Mucus	11 0
Saline substances	1 0
	100 0

The quantity of the saline ingredients in my analysis is confessedly conjectural; they have been stated by Haller to be $\frac{1}{120}$ of the whole. I have not been able to satisfy myself respecting the nature and proportion of the salts which compose this residuum: it has been said to consist of the muriate of soda, and the phosphates of lime and of soda.*

VII.

The Report of a Committee of the Horticultural Society of London, drawn up at their request by T. A. KNIGHT, *Esq; and ordered to be immediately published by the Council.*‡

The primeval state of those vegetables which are cultivated in gardens is little known.

WERE it possible to ascertain the primeval state of those vegetables which now occupy the attention of the gardener and agriculturist, and immediately, or remotely conduce to the support and happiness of mankind; and could we trace out the various changes which art or accident has, in successive generations, produced in each, few inquiries would be more extensively interesting. But we possess no sources from which sufficient information to direct us in our inquiries can be derived; and are still ignorant of the native country, and existence in a wild state, of some of the most important of our plants. We, however, know that improved flowers and fruits are

* Systeme, ix. 366. † Chemistry, iv. 613.

‡ It is printed in eight quarto pages. I shall shortly have the pleasure of giving some account of this very respectable and useful society. N.

are the necessary produce of improved culture; and that the offspring, in a greater or less degree, inherits the character of its parent. The austere crab of our woods has thus been converted into the golden pippin; and the numerous varieties of the plumb, can boast no other parent than our native sloe. Yet few experiments have been made, the object of which has been new productions of this sort; and almost every amelioriated variety of fruit appears to have been the offspring of accident, or of culture applied to other purposes. We may therefore infer, with little danger of error, that an ample and unexplored field for future discovery and improvement lies before us, in which nature does not appear to have formed any limits to the success of our labours, if properly applied.

Still greater improvements may be expected from direct culture.

The physiology of vegetation has deservedly engaged the attention of the *Royal* and *Linnean* Societies; and much information has been derived from the exertions of those learned bodies. Societies for the improvement of domestic animals, and of agriculture in all its branches, have also been established, with success, in almost every district of the British empire. Horticulture alone appears to have been neglected, and left to the common gardener, who generally pursues the dull routine of his predecessor; and, if he deviates from it, rarely possesses a sufficient share of science and information to enable him to deviate with success.

Objects of various eminent societies. Horticulture has not hitherto met with public patronage and research.

The establishment of a national Society for the improvement of horticulture has therefore long been wanted; and if such an institution meet with a degree of support proportionate to the importance of its object; if it proceed with cautious circumspection to publish well ascertained facts only, to detect the errors of ignorance, and to expose the misrepresentations of fraud; the advantages which the public may ultimately derive from the establishment, will probably exceed the most sanguine hopes of its founders.

Horticultural Society.

Horticulture, in its present state, may with propriety be divided into two distinct branches, the useful, and the ornamental: the first must occupy the principal attention of the members of the society, but the second will not be neglected; and it will be their object, wherever it is practicable, to combine both.

Horticulture: useful and ornamental

Experi-

The wild plants bear changes of climate better than the cultivated.

Experience and observation appear to have sufficiently proved, that all plants have a natural tendency to adapt their habits to every climate in which art or accident places them: and thus the pear tree, which appears to be a native of the southern parts of Europe, or the adjoining parts of Asia, has completely naturalized itself in Britain, and has acquired, in a great number of instances, the power to ripen its fruit in the early part even of an unfavourable summer: the crab tree has in the same manner, adapted its habits to the frozen regions of Siberia. But when we import either of these fruits, in their cultivated state, from happier climates, they are often found incapable of acquiring a perfect state of maturity even when trained to a south wall.

Whence the vine and peach may probably flourish here without artificial heat.

As the pear and crab tree, in the preceding cases, have acquired powers of ripening their fruits in climates much colder than those in which they were placed by nature, we have some grounds of hope the vine and peach tree may be made to adapt their habits to our climate, and to ripen their fruits without the aid of artificial heat, or the reflection of a wall: and though we are at present little acquainted with the mode of culture best calculated to produce the necessary changes in the constitution and habit of plants, attentive observation and experience will soon discover it; and experiments have already been made, which prove the facility of raising as fine varieties of fruit in this country, as any which have been imported from others.

Propagation by seed and by section.

Almost every plant, the existence of which is not confined to a single summer, admits of two modes of propagation; by Division of its Parts, and by Seed. By the first of these methods we are enabled to multiply an individual into many; each of which, in its leaves, its flowers, and fruit, permanently retains, in every respect, the character of the parent stock. No new life is here generated; and the graft, the layer, and cutting, appear to possess the youth and vigour, or the age and debility of the plant, of which they once formed a part*.

* The diseased state of young grafted trees of the *golden pippin*, and the debasement of the flavour of that fruit, afford one, amongst a thousand instances which may be adduced, of the decay of those varieties of fruit which have been long propagated by grafting, &c.

No

No permanent improvement has therefore ever been derived, or can be expected, from the art of the grafter, or the choice of stocks of different species, or varieties; for to use the phrase of Lord Bacon, the graft in all cases *overruleth the stock*, from which it receives *aliment, but no motion*. Seedling plants, on the contrary, of every cultivated species, sport in endless variety. By selection from these therefore we can only hope for success in our pursuit of new and improved varieties of each species of plant or fruit; and to promote experiments of this kind the Horticultural Society propose to give some honorary premiums to those who shall produce before them, or such persons as they shall appoint, valuable new varieties of fruit, which having been raised from seeds, have come into existence since the establishment of the institution.

Grafting does not improve the variety. Seedling plants are indefinitely various.

In the culture of many fruits, without reference to the introduction of new varieties, the Society hope to be able to point out some important improvements. Several sorts, the walnut and mulberry for instance, are not produced till the trees have acquired a very considerable age, and therefore, though the latter fruit is highly valued, it is at present very little cultivated. But experiments have lately been made, which prove that both walnut and mulberry trees may be readily made to produce fruit at three years old; and there appears every reason to believe that the same mode of culture would be equally successful in all similar cases.

Improvements as to early bearing, &c.

In training wall trees there is much in the modern practice which appears defective and irrational: no attention whatever is paid to the form which the species or variety naturally assumes; and be its growth upright, or pendent, it is constrained to take precisely the same form on the wall.

Training wall trees.

The construction of forcing houses appears also to be generally very defective, and two are rarely constructed alike though intended for the same purposes; probably not a single building of this kind has yet been erected, in which the greatest possible quantity of space has been obtained, and of light and heat admitted, proportionate to the capital expended. It may even be questioned, whether a single hot bed has ever been made in the most advantageous form;

Construction of forcing houses.

 and

and the proper application of glass, where artificial heat is not employed, is certainly very ill understood.

Application of manure to trees.

Every gardener is well acquainted with methods of applying manure, with success, to annual plants; for these, as Evelyn has justly observed, *having but little time to fulfil the intentions of nature*, readily accept nutriment in almost any form in which it can be offered them: but trees, being formed for periods of longer duration, are frequently much injured by the injudicious and excessive use of manure. The gardener is often ignorant of this circumstance; and not unfrequently forms a compost for his wall trees, which for a few years stimulating them to preternatural exertion, becomes the source of disease, and early decay.

Best varieties of soils.

It is also generally supposed that the same ingredients, and in the same proportion to each other, which are best calculated to bring one variety of any species of fruit to perfection, are equally well adapted to every other variety of that species: But experience does not justify this conclusion; and the peach in many soils acquires a high degree of perfection, where its variety, the nectarine, is comparatively of little value; and the nectarine frequently possesses its full flavour in a soil which does not well suit the peach. The same remark is also applicable to the pear and apple; and as defects of opposite kinds occur in the varieties of every species of fruit, those qualities in the soil which are beneficial in some cases, will be found injurious in others. In those districts where the apple and pear are cultivated for cider and perry, much of the success of the planter is found to depend on his skill, or good fortune, in adapting his fruits to the soil.

And numerous other fields of improvement offer themselves.

The preceding remarks are applicable to a part only of the objects, which the Horticultural Society have in view; but they apply to that part in which the practice of the modern gardener is conceived to be most defective, and embrace no inconsiderable field of improvement.

In the execution of their plan, the committee feel that the society have many difficulties to encounter, and, they fear, some prejudices to contend with; but they have long been convinced, as individuals, and their aggregate observations have tended only to increase their conviction, that there scarce exists a single species of esculent plant or fruit, which

(relative

(relative to the use of man) has yet attained its utmost state of perfection; nor any branch of practical horticulture which is not still susceptible of essential improvement: and under these impressions, they hope to receive the support and assistance of those who are interested in, and capable of promoting, the success of their endeavours.

VIII.

Account of a Machine for performing the Thread-Work in Shoe-making in a standing Posture; contrived, and for many Years constantly used by THOMAS HOLDEN, *Shoemaker, of Fettleworth, near Petworth, Sussex.**

A VERY moderate observation of the different processes of handicraftsmen will shew how extremely various are their habits of manipulating. Every different position of standing, sitting, and, perhaps, lying, may be found among them; and in works apparently of the same nature, the positions are found to be considerably unlike each other. It is probable that the first workman assumed positions which, whether aukward, confined, or inconsistent with health, or the contrary, came into universal use when the habit of the individual and the confirmed custom of master and apprentice had given them the sanction of many years. Thus we see that men's cloths are sewed by men who sit cross-legged; women's cloths by women who sit in no unusual position: turners in India hold the tool with their feet, and turn the lathe with the left hand, while they sit on the ground with the body leaning very much forward; in Europe they stand and turn with one foot, while the hands direct the tool: comb-cutters, for coarse or open-toothed combs, sit astride a large triangular stool with their work as low as the seat, so that they must keep their bodies almost

The attitudes and method of working in handicrafts are very various:

Instances:—taylors and mantuamakers; —turners in India and in Europe; —comb-cutters for fine and for coarse work.

* For which the Society of Arts gave a premium of fifteen guineas. See vol. xxii. of their Transactions. One of the machines is in their repository.

 horizon-

horizontal while at work; but those who cut ivory combs sit very nearly upright.

Other arts are also practised with injury to health.

Many other examples might be given, not only of works which are practically shewn to be capable of being executed with less injury to the comfort and health of the professors than at present; but of others where the mischiefs are no less evident, and their remedies by no means difficult, if man could be, by the gentle influence of reason, induced to depart from what habit has confirmed and made easy.

Shoemaking is an unhealthy craft.

The numerous bodily complaints which are consequent to the practice of the art of shoe-making, as now performed, are well known to all; and the remedies in this, as well as other branches of human industry, are intitled to general regard. He who improves the cultivation of the ground, or renders human labour more productive by machinery, is intitled to universal gratitude as a benefactor of the human race: He increases their comforts, and renders the means of subsistence more easy. The same argument will apply to every man who by his ingenuity or his influence shall diminish the evils which may be ultimately attendant on a life of labour. Thomas Holden has been impelled, for his own personal relief, to construct and use a simple machine for one branch of his craft. The master shoemakers and their masters, or employers, will act meritoriously in extending its use. As the editor of a Journal of *the Arts*, I have thought it my duty in this, as in most other instances, to second the views of that excellent Society, which has been so long established in London, and has so actively and constantly exerted itself for their encouragement.

It is deserving of general attention to improve the practice of the arts. Thomas Holden's machine.

Statement of its advantages.

Mr. Nicholas Turner, who addressed the Society on behalf of this machine, speaking highly in its commendation, says that its cost will not be more than from between twenty and thirty shillings. The inventor himself, after stating his sufferings from the pursuit of his business as a maker of shoes, says that he has found it to answer, and its use to have been followed by a restoration of his health. When he wrote his letter, he had made about two thousand pair of shoes with it, and he considers it as the quickest way of closing all the thread-work.

Certificates.

Certificates were also sent to the Society, from six cordwainers of the vicinity, who were witness to the use and advantages

tages of the machine; and Mr. Peter Martin, Surgeon, of Tuiborough, wrote a letter to Charles Taylor, Esq. secretary to the Society, which I have extracted verbatim.

Mr. Martin's opinion of the machine.

" I am sincerely of opinion, that Thomas Holden's invention is a desirable acquisition to men of that profession, especially to those who may be diseased internally, or who may suffer from stomach weakness and indigestion. These diseases may be aggravated, if not occasioned, by their working in a bent posture.

Bad state of the health of the inventor from practising his employment.

" The inventor, about twenty years ago, often applied to me for relief from a train of bowel complaints, and frequently had occasion to take the medicines usually employed for the relief of dyspepsia.

Perfectly remedied by using the machine.

" I repeatedly informed him, that his employment was the cause of his disorder, and desired him to relinquish it, or invent some method to do his work standing. This hint, and his corporeal sufferings, prompted to the invention. That it answers the purpose, I have reason to believe, as he and others use it. He is now free of complaints, and so improved in his corpulence and countenance, that he is not like the same man, and for years has had no occasion for medicine."

Description of the machine.

Reference to the Engraving of Mr. Holden's Invention for Shoemakers, Pl. IV. Fig. 2.

A. The bed for the closing block, and to lay the shoe in whilst sewing.

B. The closing block.

C. A loose bed to lay the shoe in whilst stitching; the lower part of which is here exhibited reversed, to show how it is placed in the other bed, A.

D. The hollow or upper part of the loose bed C, in which the shoe is laid whilst stitching.

E. A table on which the tools wanted are to be laid.

F. An iron semicircle, fixed to each end of the bed A, to allow the bed to be raised or depressed. This half circle moves in the block G.

H. Another iron semicircle, with notches, which catch upon a tooth in the centre of the block, to hold the bed in any angle

angle required. This semicircle moves sideway on two hooks in staples, at each end of the bed.

I. The tail or stem of the bed A, moving in a cylindrical hole in the pillar, enabling the bed to be turned in any required direction, and which, with the movement F, enables the operator to place the shoe in any position necessary.

K. The pillar, formed like the pillar of a claw table, excepting the two side legs being in a direct line, and the other leg at a right-angle with them.

L. The semicircle H, shown separately, to explain how it is connected with the staples, and how the notches are formed.

M. The tail or stem of the bed A, and the lower part of the bed N, shown separately, to explain how the upper part of the bed is raised or depressed occasionally.

IX.

An Essay on the Cohesion of Fluids. By THOMAS YOUNG, *M. D. For. Sec. R. S.**

(Concluded from page 88.)

VII. *Cohesive Attraction of Solids and Fluids.*

Cohesive attraction of solids and fluids.

WE may therefore inquire into the conditions of equilibrium of the three forces acting on the angular particles, one in the direction of the surface of the fluid only, a second in that of the common surface of the solid and fluid, and the third in that of the exposed surface of the solid. Now, supposing the angle of the fluid to be obtuse, the whole superficial cohesion of the fluid being represented by the radius, the part which acts in the direction of the surface of the solid will be proportional to the cosine of the inclination; and this force, added to the force of the solid, will be equal to the force of the common surface of the solid and fluid, or to the differences of their forces: consequently, the cosine added to twice the force of the solid, will be equal to the whole force of the fluid, or to the radius; hence the force of the solid is represented by half

* Philos. Trans. 1805.

the

the difference between the cosine and the radius, or by half the versed sine; or, if the force of the fluid be represented by the diameter, the whole versed sine will indicate the force of the solid. And the same result follows when the angle of the fluid is acute. Hence we may infer, that if the solid have half the attractive force of the fluid, the surfaces will be perpendicular; and this seems in itself reasonable, since two rectangular edges of the solid are equally near to the angular particles with one of the fluid, and we may expect a fluid to rise and adhere to the surface of every solid more than half as attractive as itself: a conclusion which Clairaut has already inferred, in a different manner, from principles which he has but cursorily investigated, in his treatise on the figure of the earth.

Cohesive attraction of solids and fluids.

The versed sine varies as the square of the sine of half the angle: the force must therefore be as the square of the height to which the fluid may be elevated in contact with a horizontal surface, or nearly as the square of the number of grains expressing the apparent cohesion. Thus, according to the experiments of Morveau, on the suppositions already premised, we may infer that the mutual attraction of the particles of mercury being unity, that of mercury for gold will be .1 or more, that of silver about .94, of tin .90, of lead .81, of bismuth .72, of zinc .21, of copper 10, of antimony .08, of iron .07, and of cobalt .0004. The attraction of glass for mercury will be about one-sixth of the mutual attraction of the particles of mercury; but when the contact is perfect, it appears to be considerably greater.

Although the whole of this reasoning on the attraction of solids is to be considered rather as an approximation than as a strict demonstration, yet we are amply justified in concluding, that all the phenomena of capillary action may be accurately explained and mathematically demonstrated from the general law of the equable tension of the surface of a fluid, together with the consideration of the angle of contact appropriate to every combination of a fluid with a solid. Some anomalies, noticed by Musschenbroek and others, respecting in particular the effects of tubes of considerable lengths, have not been considered: but there is great reason to suppose that either the want of uniformity in the bore, or some similar inaccuracy, has been the cause of these irregularities, which have by no

means

Cohesive attraction of solids and fluids.

means been sufficiently confirmed to afford an objection to any theory. The principle, which has been laid down respecting the contractile powers of the common surface of a solid and a fluid, is confirmed by an observation which I have made on the small drops of oil which form themselves on water. There is no doubt but that this cohesion is in some measure independent of the chemical affinities of the substances concerned: tallow when solid has a very evident attraction for the water out of which it is raised; and the same attraction must operate upon an unctuous fluid to cause it to spread on water, the fluidity of the water allowing this powerful agent to exert itself with an unresisted velocity. An oil which has thus been spread is afterwards collected, by some irregularity of attraction, into thin drops, which the slightest agitation again dissipates: their surface forms a very regular curve, which terminates abruptly in a surface perfectly horizontal: now it follows from the laws hydrostatics, that the lower surface of these drops must constitute a curve, of which the extreme inclination to the horizon is to the inclination of the upper surface as the specific gravity of the oil to the difference between its specific gravity and that of water: consequently since the contractile forces are held in equilibrium by a force which is perfectly horizontal, their magnitude must be in the ratio that has been already assigned: and it may be assumed as consonant both to theory and to observation, that the contractile force of the common surface of two substances, is proportional, other things being equal, to the difference of their densities. Hence, in order to explain the experiments of Boyle on the effects of a combination of fluids in capillary tubes, or any other experiments of a similar nature, we have only to apply the law of an equable tension, of which the magnitude is determined by the difference of the attractive powers of the fluids.

I shall reserve some further illustrations of this subject for a work which I have long been preparing for the press, and which I flatter myself will contain a clear and simple explanation of the most important parts of natural philosophy. I have only thought it right, in the present paper, to lay before the Royal Society, in the shortest possible compass, the particulars of an original investigation, tending to explain some facts and establish some analogies, which have hitherto been obscure and unintelligible.

X.

X.

Facts and Observations relating to the Theory of Heat, Light, and Combustion. By Mr. J. Arnold.

To Mr. NICHOLSON.

SIR,

IT has been said, when a person begins to theorize, there is no absurdity which he may not give credit to, and that a professed theorist is much allied to a madman. This appears partly to be true, if we consider the numerous futile hypotheses, on various subjects, which have each had its reign; each has been supposed true, and each has deservedly fallen. But when we observe farther into the matter, there is ample room to suppose, that however wild the imaginations of certain persons may have been, yet from the exuberances of some, the most important facts have been discovered. The accurate attention of a person to the most trivial subject, has led to the most important discovery: need I mention the circumstance which induced the great Newton to invent his most just theory of gravitation: If common report be true, it was the train of thought induced by seeing an apple * fall from a tree. Lavoisier, by a proper train of thinking, discovered the theory which would account for the phenomena of combustion: † and Copernicus, by attentive observation, was led to believe, that the earth was a globe; ‡ and, like the planets, revolving round the sun. Even the publisher of a false theory has often gained himself a great degree of praise, as we see in the cases of Stahl, Scheele, Des Cartes, &c. From this preface you may be led to think that I also am about to theorize and you may be induced to cry out, "*delirat, delirat!*" be that as it may, I certainly *am* theorizing.

Dissertation on the nature and effects of heat, light and combustion.

Before I begin the more immediate subject of my hypothesis, I shall take a slight notice of the new-invented field in which you and so many distinguished persons have, and still continue

* Newtonii opera omnia. † Thompson.
‡ Biographia Generalis.

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to labour; what is the origin of the science? It has no pretensions to antiquity; it was not a part of the learning of the antients; the Greeks *did not* cultivate it; the Romans were unacquainted with it; but the Egyptians! the people of that country were great magicians, and could perform works which persons of other countries were unable to effect; they are supposed to have had converse with devils. What is the meaning of the new art? to what part of nature does its actions belong? It is called chemistry, an art which relates to the formation of medicines to prolong the life of man; but it has improved but little in this respect: formerly it was dignified with the name of alchemy, * a most august and reverend art, the votaries of which attended to the transmutation of metals, and attempted to discover the art of making gold, as well as of forming an universal medicine,† by which riches would abound, and mankind would dispense with that last distressing rite which hitherto has been performed by all, and to which all alive must bend.

Such a description, a few years since, might have been given of our art; but since that time, to what an extensive expanse has it reached! to it all nature is subject, whether animate, or unorganized; it is a science which treats of the minute ‡ particles of matter, and of the changes which take place upon applying different particles to each other.

The different substances in nature may be divided into several orders, solids, fluids, gases, and § unconfinable substances. The three first are always cognizable to two senses; the latter are apparent to one sense alone, viz. light to the sense of vision, and heat to the sense of touch

With regard to our speculation, the last set of substances are most to be attended to, though to explain their effects, the other substances must be had resort to. One of the most wonderful phenomena in nature, but at the same time the most familiar, *videlicet*, combustion, has been attempted to be explained at various times, by well-adapted theory. In this inquiry, and as inventors of different hypothesis have appeared at various times, Hooke, Mayow, Beecher, Stahl, Macquer,

* Albertus. † Paracelsus. ‡ Fourcroy. Heron.
§ Thompson.

and

and, at length, Lavoisier. In the remotest ages fire was supposed to be a substance which, by being applied to certain other substances, devoured them, and what was left was supposed to be unfit for the * food of fire: to this succeeded the opinion that a † solvent acting rapidly on the combustible, was the cause of the evolution of heat and light; to this the supposition that violent friction and ‡ agitation between the combustible and a matter existing in the air was the cause of the phenomenon. This was followed by the hypothesis that inflammables had the peculiarity of § running into violent whirlings, by which combustion was produced; the next opinion was that light existed in a || dense state in all inflammables, and by certain actions was set free; afterwards a peculiar very subtile,** most elastic fluid, was supposed to be condensible, in certain bodies, from which it escaped, and produced certain appearances, among which was combustion. This hypothesis was superseded by the opinion that heat and light were evolved by the †† air, which combined with the inflammable body, at the same time giving out a certain principle which rendered the air afterwards incapable of supporting combustion; after this combustion was supposed to depend on a substance which was the same in all combustibles ‡‡. This theory was the last to overturn, and was succeeded by that which stated that during combustion oxigen gas was §§ always absorbed. This theory is certainly the best, and appears to have a firm foundation, but it evidently fails in explaining the most striking phenomena of combustion, viz. the |||| origin of the heat and light, which is the very essence of combustion. It is the province, therefore, of this paper to explain the probable sources of these substances; though, from the extreme intricacy of the subject, it is with great diffidence that I do it.

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It may be proper, before immediate procedure to the business, to notice those substances which are necessary to constitute combustion. Authors inform us, that oxigen gas and an

* Albertus Magnus et alii. † Hooke. ‡ Mayow.
§ Stahl Scheele et alii. || Macquer. ** Newton.
†† Crawford. ‡‡ Kirwan. §§ Lavoisier.
|||| Murray et alii.

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* inflammable substance are necessarily present in every case of combustion, but in the present speculation I am under the necessity of supposing both oxigen gas and combustibles compound substances, so that we have four substances present in every case of combustion,—oxigen, light, the base of an inflammable substance, and heat.

In speaking of caloric and light, we do not think it necessary to enter into the subject minutely, but only to mention those circumstances respecting them which are allied to our theory. With regard to the mode of existence of heat, there is still some dispute among philosophers, some supposing that it is a property produced by the motion of a very subtile æther † which pervades all space; others, that it is a real substance, sometimes giving a sensation to the sense and touch, but often so hidden ‡ in bodies, that its presence cannot be perceived. Perhaps the latter opinion is the best, for by it we are the better enabled to treat of the subject. As I mentioned before, this substance is capable of entering into chemical union with certain other substances; in which case it can only be discovered by certain properties which those substances possess, and this property is that of inflammability; here therefore I differ in opinion from most philosophers, who suppose that heat exists latent in greatest quantity in certain aerial susbstances, as oxigen, carbonic acid, &c. § which I profess to have none, unless that which regards the temperature of such bodies, for I am not inclined to deny the circumstance that different substances require different portions of heat to make them of an equal temperature; on the contrary, I suppose that the experiments of Crawford, Irvine, &c. with respect to the capacities of bodies for heat, are as accurate as the subject will allow; but the heat, which I call latent, has no effect in increasing temperature; a quantity of sulphur at *plus* one hundred degrees, contains as much, and no more, of that latent heat, than the same portion would at the most intense cold ever observed.

Perhaps it will be better to state in what the peculiarity of my opinion consists. According to the writings of the

* Thompson et multi alii. † Newton. Leslie. ‡ Black.
§ Crawford.

most

most celebrated chemists, caloric is of two kinds; one of these is called caloric of temperature, which gives the sensation of warmth; the other is hidden in substances,* so that its presence cannot be perceived but by certain changes which such substances may be made to undergo; thus we find that ice at 32° contains much less heat than water at 32°,† though to the sense of touch they are the same. The third state in which I suppose heat is capable of existing, is as a component part of all inflammable substances, and is that substance on which their combustibility depends; and this I think it necessary to take for granted, to elucidate the phenomena of combustion, which I hope to do in this imperfect memoir; but as it is a thing necessary, for the truth of every theory, that the data on which it is founded should have truth for their basis, it may be prudent to state the grounds on which the present assumption is settled, especially as it is a common fault with speculators to build castles without examining the foundation which is to support them, and hence the superstructure being well increased, or perhaps nearly finished, by a fault in the foundation, has soon fallen prone; as has been observed in those buildings which were raised by Stahl, Scheele, Des Cartes, &c. though, at first sight, they appeared very fair, and exceedingly strong.

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Since the reception of the Lavoiserian theory of combustion, some philosophers have supposed that the evolution of caloric and light was from the oxigen gas alone,‡ others have supposed that light was afforded by the § combustible substance, and heat by the oxigen gas,|| which last opinion has had the greater number of follo ers, Some unable to account for the circumstance have supposed that light and heat are only different forms of the ** same substance. Lavoisier appears to have inclined to the opinion that the light was afforded by the †† oxigen gas, though he also supposed that the heat also might have the same origin. Before I

* Dr. Black et alii. † Dr. Black. ‡ Lavoisier, Brugnatelli, &c.
§ Maquer, Richter, De la Metherie, Chenevix, Thompson, Gren, et alii. || Thompson, et cæteri. ** Murray's Chemistry, p. 178.
†† Fourcroy.

state

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state my objections to these opinions, I shall proceed to notice light, oxigen, and inflammable bodies.

Light, like heat, according to some, is a property caused by the vibration of a very subtile fluid which fills all space, and which undulation * is produced by the sun, and other luminous bodies. According to others, it is a real substance, emanating from luminous bodies in strait lines, † and which by approaching our eyes gives the sensation of light. In obedience to this, therefore, I shall suppose it a substance, and like heat, capable of existing in a latent sensible form. The first is that which affects our organs of vision, the second as a component part of a substance, with which it is in chemical union, but from which, by the action of certain substances on it, may be expelled and rendered sensible. Whether light be able to combine with more substances than one, I cannot at present determine, but from many appearances, which are familiar to chemists, I am apt to believe that it is.

Inflammable substances have been usually considered as elements, but I am under the necessity of calling them all compounds, ‡ each consisting of a base and the condensed matter of heat. This opinion indeed, does not appear to be peculiar to myself; for an excellent chemist, who has published an extensive chemical work, appears to think them binary compounds, as consisting of a base and light. Of these substances there are various kinds, each elementary combustible, consisting of its peculiar base; but they all resemble each other in this, that each one contains in composition the condensed matter of heat, but that in different degrees of cohesion, some very easily part with it, as sulphur, phosphorus, &c. but from others it can scarcely be expelled, as the metals. Some philosophers have supposed that these substances consist of a base, and the matter of *light*.

Oxigen gas has been by many supposed to be a compound of heat; and a base, § some have reckoned it a ternary compound, consisting of a base, heat, and light; || but I, as was before observed, take it to be a combination of the condensed

* Huygens. Euler. † Newton. ‡ Thompson.
§ Murray et alii. || Brugnatelli, Lavoisier, &c.

matter

matter of light, and a base: when I speak therefore of oxigen, I mean the base of oxigen gas, which perhaps can only exist in combination with the base of an inflammable substance, thereby forming a product of combustion.

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Now therefore let us examine some cases of combustion, to observe whether our theory will apply, and it will be preferable to begin with the most simple. Water is known, (a product of combustion) to consist of oxigen and hydrogen, or rather of the bases of oxigen and hydrogen gases, and consequently the heat and light were evolved in the conversion of these gases into water. It therefore must be apparent to all, that as oxigen and hydrogen gases consist of four substances, and water of but two, that to reproduce the gases from water, the light and caloric must be added; but it may be said that heat and light will not unite with water, and convert it into its component parts; if we apply water to the strongest light and heat, it will only be converted into vapour. To this I answer, that the heat and light in the gases exist in the latent form, and we can only cause them to combine with water by presenting them to it in such states. But how can we procure such light and heat, they are incognizable to the senses, and therefore must be imperceptible? *I hold electricity to be light and heat combined, and capable of effecting this change.* When I speak of electric matter, I mean, that though light and heat are generally apparent on electric matter passing from one body to another, yet that this light and heat are not necessarily in a sensible state, for electricity has the power of entering some substances without producing a shock, or evolving light and heat: to prove this, we will make some observations upon the effects of electricity on combustible substances, and products of combustion. Combustible substances, according to our theory, are binary compounds, consisting of a base and heat; to expel this heat it is necessary to present to them a substance known by the title, oxigen gas, which is a combination of a base and light. A mixture of these two substances may therefore be supposed to be saturated with the matters of light and heat, but by an increase of temperature a divellent attraction takes place, the base of the oxigen having a greater affinity for the base of the inflammable than

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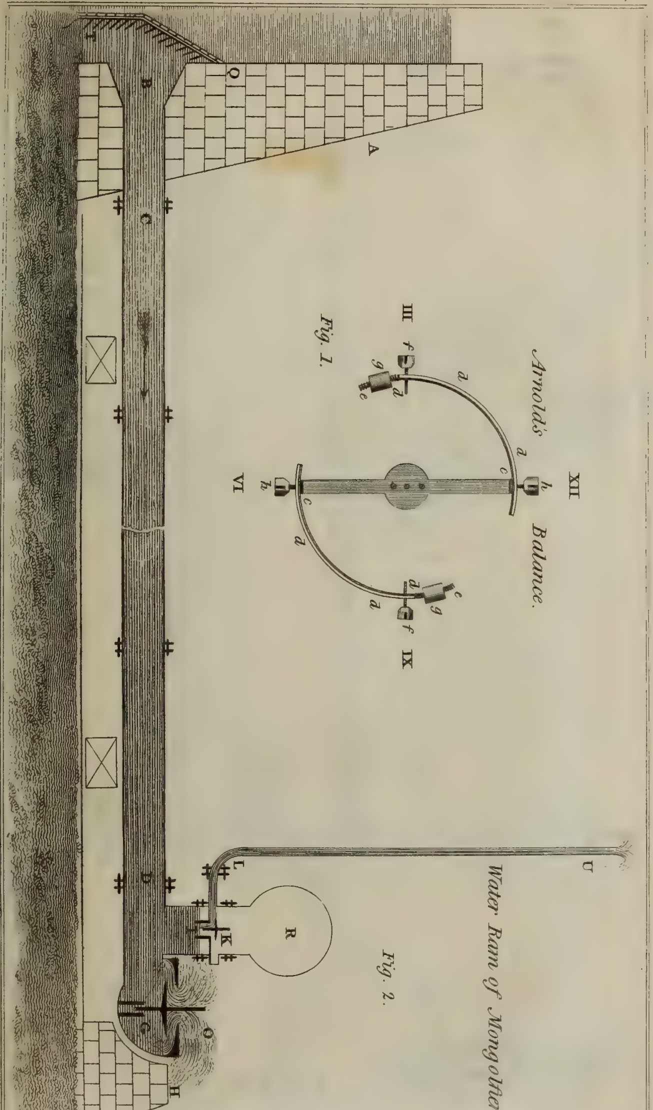
than each has for its heat and light, which therefore, from their unconfinable nature, escape. The action of electric fluid, on a combustible substance, is only to increase its temperature, and to produce combustion; for the substance is already saturated with that matter which electricity offers to it. But let us present to the electric matter a substance of a different nature, a binary compound, consisting of the bases of oxigen gas and an inflammable substance, or what is called a product of combustion; here the action is widely different, no heat is evolved, no light appears, but the substances received the electric matter in a latent state, and is resolved into two binary compounds, an inflammable substance, and oxigen gas. This process may be exemplified, in a common case of combustion, in which both the necessary compounds are in the gaseous form. If a certain quantity of oxigen gas be mixed with hydrogen gas, we shall have the four substances necessary for combustion, oxigen, light, the base of an inflammable, and heat. The electric spark being applied to this mixture, causes the bases of the oxigen and hydrogen gases to unite, and the light and heat to escape; here therefore we have the product of combustion, which is a substance void of light and heat, and it will be necessary to offer light and heat to it, to reproduce the oxigen and hydrogen gases; but we may offer sensible light and heat to them in profusion and no union will be produced; if we heat it ever so greatly it will only be evaporated. Heat and light exist in the gases we have mentioned in an imperceptible form, and to induce an union, we must offer them in the same state. This form of light and heat is nothing else than the electric fluid, and hence, by passing electricity through the water, it is resolved into its primitive gases.

Electric matter, as it is produced from the common machine, is of a compound nature, a combination of the matters of heat and light; being therefore introduced into water, myriads of minute bubbles escape, which are oxigen and hydrogen gases *mixed together*. But of late years a different modification of the electric matter has been observed, in which the principle may be divided, with matter of heat escaping at one point, and the matter of light at another. So that by introducing the points of the galvanic machine under


Arnold's Balance.
Fig. 1.
XII
III
VI
IX
Water Ram of Mongolfier:
Fig. 2.

Mr. Holden's Machine for Shoe-makers.

Galvanic Apparatus by Mr. Sylvester.

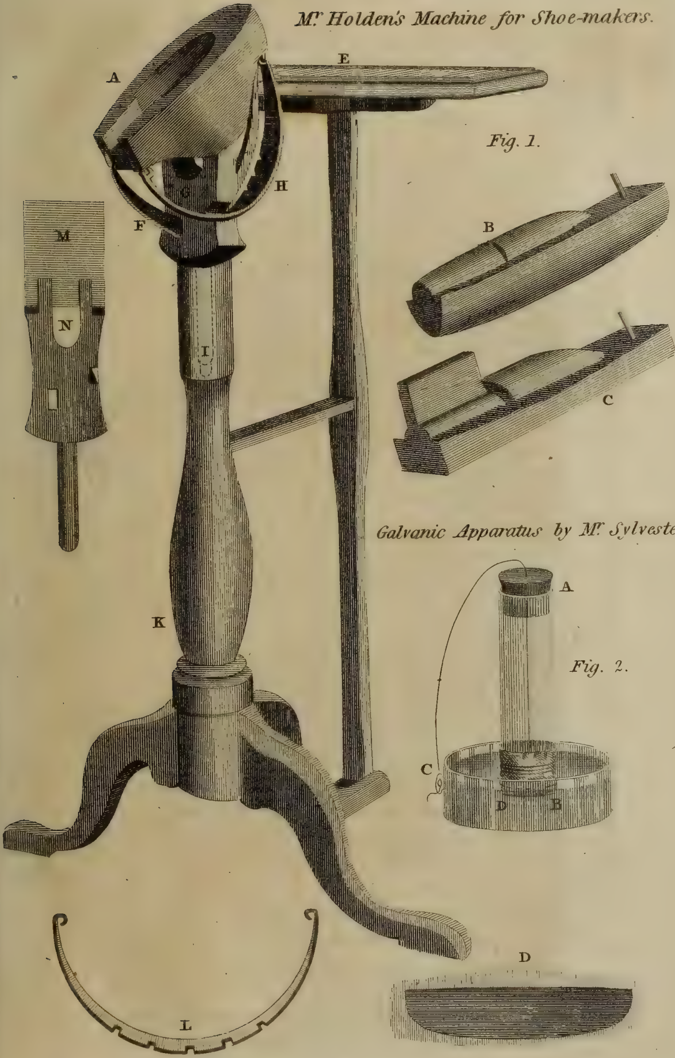


Fig. 1.

Fig. 2.

under water, we may obtain the oxigen gas from one point, and the hydrogen gas from the other; and this appears to me to be caused by the base of the oxigen gas existing in the water combining with the matter of light from one end of the trough, and the matter of heat uniting with the base of the hydrogen gas at the other.

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With galvanism we appear to be but very little acquainted; we are shown its powerful effects in burning substances, which were before supposed uninflammable; we have exhibited to us the powerful effects it has upon the vital animal fibre, but no one has attempted to explain the causes of these effects; the cultivators of it appear to have explored in the dark, making numerous experiments, and wondering at their results at the beginning of an experiment, unknowing what to expect; and, having finished it, unable to account for the change: like the practice of the empirics of old, who were employed in obtaining experience by actual observation only, unassisted by reason or theory. If a disease disappeared under the use of a particular remedy, that substance was a cure for the complaint; if a person was affected with purging after swallowing a certain article, that article was set down as of a purgative nature, when perhaps neither of these effects were really caused by the substance employed, the person recovered by the *vires medicatrices naturæ*, and the purging was caused by a substance prepared by the body itself; and it remained for future experience to prove the fallacy of the unjust account. Just so it is with galvanism, by passing the influence through water, oxigen and hydrogen gases appear; it is therefore set down as a fact, proved by experience, that galvanism decomposes water; and, if galvanism has the effect of decomposing one substance, and has no effect on another, these circumstances are related in the empirical account, and no one endeavours to seek further into the subject, no one endeavours to explain the causes of these occurrences.

If I may hazard an opinion, galvanism differs from common electricity in this: in the latter, the influence escapes from one point only, and is thence a compound matter; but in the former it escapes by different points. I have said before, that electric matter is a combination of light and heat in

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in a pure and detatched state, and capable of entering certain substances, without affecting either the organs of vision or of touch, as is observed when it enters a product of combustion.

The effect of galvanism varies with different inflammable substances according to the intensity of heat which is necessary for their combustion; phosphorus, hydrogen, carbon, and sulphur, inflame at low temperatures, and therefore are immediately set on fire; but the metals which I consider as inflammable substances, are more difficultly ignited, and are capable of retaining a considerable quantity of the electric fluid without undergoing change; but even these are inflamed by a powerful instrument.

In treating of combinations of oxigen with other substances much ambiguity has arisen among chemists; for no one can suppose that the oxigen which is contained in the products of combustion is the same as oxigen gas, which is a supporter of combustion. An ingenious philosopher of Italy * observing this circumstance, has endeavoured to remedy it by supposing oxigen able to combine with substances retaining its heat, as in nitric and oxi-muriatic acids; and in its simple state as in the sulphuric and carbonic acids, but as he supposed that heat and a base were the component parts of oxigen gas, he called it thermoxigen, as retaining its heat; but, as I have differed from him in supposing it a compound of light and a base, I may call it photoxigen.

It is a question which, I must confess, I am unable to resolve, whether the matter of light can exist in substances without the presence of oxigen? But thus far may be said, that oxigen and light are capable of combining with other substances, and thereby may be in a dense form, as is seen in the acid supporters, and certain metallic supporters. Nitric acid therefore we may state as a combination of oxigen, light, and another substance called azote, which has hitherto been called an element; but whether it is the base of any substance, or can be united with the matters of heat and light, I am unable to determine. It has long been known that the component parts of nitric acid were the same as that of atmospheric air, but that the proportions of different sub-

* Brugnatelli.

stances

stances varied. I am induced to think that the change consists principally in this, that nitric acid contains a larger portion of condensed light than atmospheric air, which may be supported from an old and * celebrated experiment, which has been often repeated, and with the same result. A quantity of atmospheric air was inclosed in a proper vessel, and frequent electric sparks were passed through it, till at length its bulk diminished, and it had lost its properties, having manifestly become nitric acid. But the most remarkable part of the experiment is the appearance of the electric matter which acts upon it: as I stated before, electricity is a compound of heat and light; but there are certain substances which can only unite with one of these, which is the case with the experiment now spoken of; the light is absorbed by the mixture, and the heat evolved, for no sparks are perceived, but a considerable quantity of heat is evolved in a sensible form. Many may suppose that this heat escapes from the condensation which occurs, and that the capacity of the substances are changed. One of these opinions appears as likely as the other; and I think mine is the more preferable, for the condensation of gases does not necessarily evolve heat, as we observe on presenting gaseous muriatic acid and ammonia to each other.

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As azote is called a simple elementary substance which is uninflammable, so we have another which has the same title and property of incombustibility, *videlicet*, the muriatic acid; and this, like atmospheric air, appears to be capable of uniting with the matter of light, and thereby becoming a supporter of combustion. The real combination of this acid has not been stated, though I have no doubt that the experiments which have been made, in Italy† have great weight in leading us to believe that it is a combination of oxigen and hydrogen, and may be supposed a product of combustion. Besides the supporters of combustion we have already mentioned, there appear to be others which deserve that name, which are certain metallic oxides, especially the black oxide of manganese, and the red oxide of lead.

* Cavendish. † Pacchiani.

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Before going farther, it may be necessary to give some examples of the changes which galvanic influence produces on substances, the nature of which, with respect to combustion, are very different. These substances may be arranged under four heads, supporters of combustion, products, inflammable substances, and detonating or deflagrating substances. One example, with regard to a supporter of combustion, I have given in treating of the conversion of atmospheric air into nitric acid. It now remains therefore to make some remarks on muriatic acid. I do not know whether the same experiment has been made on this substance as on atmospheric air, but I am induced to think that a similar change would be observed, *scilicet*, that on passing the electric fluid through it, light would be absorbed, and heat evolved, the acid thereby becoming a supporter, or what is commonly called oxigenated muriatic acid. I leave this to future experimenters to determine. It appears to me, that oximuriatic acid is always obtained by a process analogous to that of passing electricity through muriatic acid; a quantity of a substance containing light in a dense state is presented to it, and unites with it in precisely the same manner as electrical light would. Black oxide of manganese, a supporter of combustion, a combination of light, oxigen, and the base of an inflammable substance or a metal, is presented to the muriatic acid, consisting of the bases of oxigen and hydrogen gases, and which, at the same time, is capable of absorbing and retaining the matter of light, which it, in effect, receives from the black oxide, and thereby becomes a supporter. In this instance we manifestly have a translation of that substance, on which combustibility depends, passing from the oxide to the muriatic acid, so that what was before an incombustible becomes a supporter, and that which was formerly a supporter becomes an incombustible. What name can we give to this chemical action? is it semi-combustion? the heat only escapes! In all real cases of combustion light accompanies it, in this it is retained.

The most common supporter of combustion is oxigen gas, a combination of oxigen and light: other supporters are ternary compounds; oxigen, azote, and light, in the nitric acid, and oxigen, the base of an inflammable, and light, in the oximuriatic acid; the metallic supporters are combinations of light,

oxigen,

oxigen, and a metal. Products of combustion are all combinations of the bases of oxigen gas, and an inflammable substance. Hence, as they are bereft of that light and heat which they possessed before combustion, it is necessary to afford light and heat to them to obtain a decomposition, and this decomposition is effectually obtained by offering electric matter to them, as *it* is that form of light and heat with which they can combine. One example of this decomposition has been mentioned in a former part of this paper, in which it was shown that water is convertible into its primitive gases; we shall take another instance of a different substance, namely, sulphuric acid, which is a product of combustion, and possesses most active properties. On introducing the wires of the galvanic pile into a vessel containing this acid, it is soon decomposed, as is the case with water, oxigen gas appearing at one extremity and inflammable sulphur at the other, and at the same time we observe little or no changes of temperature, as is the case with water, for it absorbs both principles of the electricity.

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The third set of substances on which the influence of galvanism has been tried, are inflammables; these are all compounds of heat and a base, but they differ in this, that their principles vary very much in the degree of cohesion which exists between them, some are separated by a *little* increase of temperature, others scarcely at all by the highest temperature which we can apply; but to produce this decomposition it is always necessary that a substance should be in contact with them, which contains the matter of light as a component part; and this, in most cases, is oxigen gas. As an instance of the influence of galvanism on this set of substances, we may mention charcoal, which is soon ignited, by which it parts with the heat which it contained, and absorbs a principle from the air called oxigen, which at the same time gives out light, which usually exists in composition with it, in its aerial form. To mention another instance, we may take a subject more difficultly inflamed, viz. iron; this, on being presented to the influence, if it be in any considerable quantity, is only rendered red hot, but by being in the form of small wire it burns with very brilliant sparks and great heat; the product which is obtained from it is an oxide of iron.

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The next set of substances which we may notice, is what have been called deflagrating or detonating substances, these are necessarily quaternary, many of them quinary compounds, and contain the four substances necessary for combustion, and are hence inflammable in close vessels, without being in contact with oxigen gas, they contain a supporter and an inflammable, or, as we may otherwise speak, oxigen, light, the base of an inflammable, and heat. A mixture of oxigen and hydrogen is an example of this kind, and so is gunpowder, the ammoniureta argenti, and auri, and other fulminating compositions. These immediately explode on the approach of the electric fluid, and two of their component parts escape, viz. the light and heat. One farther instance I shall mention of the action of a supporter of combustion on an inflammable substance. Oximuriatic acid being mixed with ammonia, both, in their gaseous forms, have a remarkable action on each other. The first is a compound of hydrogen, oxigen, and light; the latter of hydrogen, azote, and heat. In this case an attraction exists between the bases of the combustibles which form the muriatic acid and the ammonia, the heat and light escaping, as well as the azote, which is set free. From this experiment it may not appear strange that muriatic acid has been detected in passing galvanism through water.

We have now given our opinion with respect to the nature of the electric and galvanic fluids, and endeavoured to prove the truth of it from certain effects, which it produces on different substances; from hence it may appear that it is entirely dependant on chemical changes, and upon the action which different bodies have on each other. It appears to me, to be nothing more than an action of a supporter of combustion on a combustible body; for we find that in every galvanic apparatus there is an oxidable metal, a substance capable of oxidating it, and what may be called a conductor, which has the power of conveying off the electric matter produced by this chemical action, and it remains for future experiments to prove that every such chemical action really does produce electric matter, which if performed in proper apparatus might be as evident as that from the trough or pile.

From

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From these transient remarks it may appear evident to any one employed in galvanic research, what is the true action of this wonderful chemical agent: its nature also I hope has been somewhat elucidated. And as a path has been shown to lead on persons labouring in the field, they will be more sure of employing themselves to advantage, whence they may have a probable supposition of what will be the result of their experiments. If the influence be applied to an inflammable substance, inflammation will ensue, if to a product of combustion, it will be resolved into oxigen gas and an imflammable matter. If to a substance capable of becoming a supporter, *it* will absorb light and give out heat.

It is a question, and I cannot say whether it has been solved, whether the condensed matter of light can exist except with a necessary quantity of oxigen to retain it; or whether it can be present in any substance which does not contain oxigen? I believe that nothing has been found as supporter of combustion unless it contains oxigen gas as the nitric and oxi-muriatic acids. Would it be possible to form an oxi-sulphuric acid, or an oxi-nitric acid, by help of the galvanic apparatus? Those salts, which contain the nitric or oxi-muriatic acids, have been called detonating salts, perhaps improperly, for but few of them detonate without the addition of an inflammable. The nitrous ammonia is a real detonating salt.

Among the discoveries by the galvanic instruments, I believe no one has decomposed the carbonic acid or the phosphoric acid: by proper management it may be done, and with regard to the first, some very curious appearances may be observed. Carbonic acid and water are products of combustion, but if they are combined with that substance on which the inflammability of bodies depend, they become ardent spirit or oil. There appears a gradation of changes between gluten farina, saccharum, alkohol, and oleum. Any of these give a product of water and carbonic acid.

It may now be proper to notice the objections which have been made to the opinion, that light is afforded by oxigen, and heat by the inflammable body during the process of combustion. The much lamented French chemist who was the inventor

Dissertation on the nature and effects of heat, light, and combustion.

tor of the present theory, was much inclined to the opinion that light was afforded by * oxigen gas; but he appears to have supposed that the heat had also the same origin. A very celebrated chemist of Italy † also has published the same opinion. Succeeding chemists, however, have been led to suppose, that the light was afforded by one of the substances necessary for combustion, and heat by the other, and for certain reasons they have attributed the heat to the oxigen, and the light to the inflammable. These reasons therefore, it behoves us to scrutinize and try the weight they have in this affair.

Primo.—Bodies in the aeriform state contain in equal weights at the same temperure much more caloric than fluids or solids do. This assertion is true in one sense; a pound of steam at 212° contains much more latent caloric than a pound of water at 212°, in the same manner as a pound of water at 32° does than as much ice at the same temperature. But this rule does not hold with substances of different *natures*, otherwise a gaseous substance could not be absorbed by a fluid or solid without a great increase of temperature. But if we add a very small quantity of water to a large portion of ammonical gas, the ammonia is immediately absorbed, but the sensible heat is but little increased; or if we mix muriatic acid gas with ammoniacal gas, a solid substance is immediately formed, with but little increased temperature. The same also is observable on the mixing of carbonic acid gas and ammoniacal gas, the heat in these cases can entirely be accounted for from the different capacities which these bodies have for caloric.

Hence I think we may lay aside the opinion, that the condensation of an aereal substance is the cause of the evolution of heat. For if this were the case, a substance receiving gaseous form would always produce a decrease of temperature; but we do not find that the case, when carbonic acid gas is evolved from lime, or when a large quantity of gas escapes in the deflagration of gun-powder or other substances of the same nature.

* Lavoisier, Fourcroy. † Professor Brugnatelli.

Secundo.

Secundo.—If the light were afforded by the oxigen gas we should have most when the greatest quantity of oxigen disappears; but the light is greater in the combustion of phosphorus, than in that of charcoal, and still greater than in the combustion of hydrogen gas; but more oxigen gas disappears in the combustion of the hydrogen gas than in the other two. We have no accurate photometer by which we can measure the intensity of light, and even granting that 1000 times as much light appeared in the combustion of phosphorus as in that of hydrogen gas, no one can affirm that the light on inflammation does not bear a ratio with the intensity of heat, and we find that the heat evolved in the inflammation of hydrogen gas is but small when compared to that of phosphorus or charcoal. Light does not appear from the extremities of the galvanic pile, unless they be connected, and the heat and light conjoined. Hence we find the greater is the intensity of the heat in combustion, so also the light is increased in the same proportion.

Dissertation on the nature and effects of heat, light and combustion.

Tertio.—By the combustion of hydrogen gas and oxigen gas from the new invented blow-pipe, we obtain a more intense heat than could have been done before, except by mirrors or lenses, but we observe but little light in this case. The heat produced from this blow-pipe is by no means so intense as many have supposed. Its effect are tried upon inflammable substances, iron, copper, and which are themselves combinations of considered heat; in these cases therefore we have the heat of the hydrogen gas, and that of the inflammable substance, acting together, and must consequently have very great heat. I believe, that by passing a stream of oxigen gas through the flame of spirit of wine or oil, the heat might be as great or greater. I have observed the action of the common blow-pipe on a stick of glass, and it appeared nearly as great as that of the hydrogen and oxigen gas blow-pipe: quite so great for the reasons before mentioned, it could not be.

These, I believe, are the three principal objections which have been stated against the doctrine which it is here attempted to support. It would take up too long a time to mention all which are to be met with in authors.

Dissertation on the nature and effects of heat, light and combustion.

With regard to the objections which may start against my opinion of the electric fluid, two at least have occurred: the first is, that if the galvanic influence were nothing more than the combined matters of heat and light, every case of combustion would be an electrical experiment. To this I may answer, that from substances in combustion the light and heat go off in the sensible form; but it is very different in the galvanic light and heat, the essence of *it* consists in this, that it is not in its sensible form; as soon as it affects the eye, or the thermometer, it is no more electric matter, but common light and heat; by how much light and heat is given out from an electrified substance, by so much is that electric matter diminished in quantity. Galvanism is produced by an action very similar to combustion; a substance is oxidated as is the case with combustion, but the heat being insufficient, and the substance present varying somewhat from those producing real combustion, heat and light are given out in that peculiarly subtile form which characterizes the electric fluid. A supporter of combustion in both cases acts on a combustible substance, the former of which in both cases gives out light, and the latter heat, and these moving in different directions through the apparatus, at the place of contact, appear in the form of sparks.—Hence it may appear that galvanic troughs may be much improved, for the action appears to be proportionately great, according to the surface of the oxidable metal which is presented to the oxidizer.

J. ARNOLD.

May 4th, 1806.

XI.

On the Sugar of Grapes. By PROFESSOR PROUST.*

Sugar of grapes.

MY assistant, after several days occupation in drying grape sugar which had been drained of its melasses, has succeeded in

* Journal de Physique, lxi. p. 399.

in rendering it as perfectly white as that of my former experiments. I have remarked, that the sugar which forms granular crystals is easy to be divided, and yields readily to the operation of claying: but it is not so with what remains thin, fat, like honey, and consequently imperfectly freed from its syrup; the humidity of the argil penetrates it too slowly, and it dissolves and carries away too much sugar. All the efforts of the sugar-baker should therefore, in my opinion, be directed towards obtaining the crystallization in the most granulated state possible; and I am encouraged to hope that the difficulties which may appear in the way of our object will be surmounted, as grape sugar crystallizes considerably quicker than that made from cane: The candy which we see so frequently on sweetmeats of every kind affords a daily proof of this. All that I have examined for several years past in the confectionary of my own house, was a sugar perfectly analogous to that from grapes, without admixture of that from cane. The latter is therefore much less crystallizable. Sugar of grapes.

Grape sugar is not so white, I must repeat it, as that from cane; but its flavour is full, pure, and without the least remains of a vegetable taste or smell.

XII.

SCIENTIFIC NEWS.

Magnetical Telescope.

MR. Edward Troughton has constructed a new telescope for determining the magnetical meridian. It consits of a tube of steel, containing a set of lenses with cross wires or spiders weebs, in the usual manner. It will easily be understood that an instrument of this kind, after receiving the magnetic power, may traverse upon pivots or by any other similar mode of suspension, and will dispose itself in the magnetic meridian. One of the difficulties attending the magnetic bar of the usual form is, that its line of direction may not be parallel to its side; and it is not easy to determine the quantity of error, by re- Mr. Troughton's invention of a magnet in the form of a tube fitted up as a telescope.

Magnetical telescope.

versing, because this last operation is, in most cases, impracticable. Mr. Troughton's magnetic telescope may be turned round in its support like that of the levelling instrument; and it will determine the magnetic meridian, whenever any one and the same distant object is seen, upon the centre of the cross wires after the telescope has been turned half round on its axis, as in its former state. By this contrivance the diurnal and the other variations, to which the magnetic bar is subject, may be easily observed, and it may even be ascertained whether the direction of the magnetic force varies with regard to the axis of the tube. As the present notice is intended to be short, and as I hope for farther communication from the inventor, I forbear to enter upon observations respecting the kind of instrument to which this telescope may be attached. It is evident that observations for the dip and variation may easily be made by reference to the plumb line, and to the heavenly bodies.

Mr. Troughton is already engaged in the executing orders received for this instrument, as well from scientific men of this kingdom, as from those on the Continent. The learned reader will recollect several instances, in which the eye-peice of a telescope, applied to its focal image, has, with great convenience and precision, afforded angular determinations which could not with the same convenience have been observed in the usual methods. The application of a magnifier to the extremity of a magnetic needle is also attended with difficulties, which Mr. Troughton's invention will obviate; at the same time that it facilitates and extends our views of a very curious and useful natural power. Among those active philosophers who do not wait for the construction of instruments, or cannot afford to make purchases for all their several occasions, this contrivance will suggest experiments, which may be made by tyeing together a telescope and a magnetic bar, and suspending them by a thread or fine wire, for some of the purposes of observation which they may be disposed to make. This, though a clumsy instrument, will also bear reversing, and may be usefully applied.

New

New Metal Columbium

The learned Millin, member of the French Institute, in his Magasin Encyclopediæ, for December last, page 388, relates the following particulars concerning the mineral to which Mr. Hatchett has given so much celebrity; (see our Journal XX. 235). They were communicated to him by Mr. Valentin, physician at Marseilles, who is well known as a cultivator of natural philosophy and history. **Ore of columbium.**

The mineral examined by Mr. Hatchett was found in a spring in the American province of Massachusetts. The spring is in the town of New London, in the state of Connecticut. It is near the house where Governor Winthorp lived, at the distance of about three miles from the sea up the harbour. The place was formerly called Nantneague. Mr. Francis B. Winthorp, of New York, has obligingly forwarded to the Historical Society of Massachusetts, the manuscript paper of his ancestor relative to the place, and the minerals which he presented to Dr. Hans Sloane, at London. It is to be hoped, says our author, that other specimens of this mineral will be obtained.

Sudden Eruption of Water near Como.

A remarkable phenomenon has excited the curiosity of the inhabitants of the vicinity of Como. In the commune of Laorca, in the territory of Alleco, a subterraneous spring all at once burst forth, which immediately overthrew two houses, and in the course of fifteen or twenty hours a forge which stood in its way. This spring is loaded with a thick chalky matter, which mixing with the water has rendered the lake into which it falls quite turbid. M. La Carte, officer of engineers, who visited the spot, attributes the accidents which have happened to a subterraneous excavation made by the water, and he judges that the extent of further damage will depend on the actual magnitude of the cavity. **A new spring in Italy.**

Prussian

Prussian Academy.

Sittings of the Prussian Academy. Prize questions.

The Royal Academy of Sciences at Berlin, in their public sitting of last August, gave an account of the competition for the prizes for 1804. The question relative to the law of Mariotte not having been satisfactorily resolved, was deferred to this present May. The same measure was adopted with regard to their question concerning the structure of the lungs. The third question on the inflammation of the spleen was resolved in three memoirs: The prize of fifty ducats was adjudged to Mr. Klausch, physician at Militsch in Silesia.

Seven memoirs were received on the philosophical question concerning analysis and the analytical method. That of Mr. Francke, rector of Husum, obtained the prize. The question by an anonymous proposer of last year, "*Why civilization proceeded from the East*," produced several memoirs; but the decision of the academy is deferred till they shall have been examined.

The class of philosophy has proposed for the year 1807, the following subject for a prize:

"Does there exist an immediate internal perception, and in what respect does it differ from intuition and the simple abstraction by the rules of thinking and perceiving.

"In what respect do intuitions differ from sensations and the intimate sense.

"In what relation do these actions and situations of the intellect stand with regard to conceptions and ideas."

Poland

Prize questions.

In the month of June last, the class of Physical Sciences of the University and Imperial Academy of Vilna, proposed the following prizes:

"Besides the diabetes mellitus of medical writers, are there other maladies peculiar to man, which from decided experiments are known to produce in different organs a secretion similar to that of saccharine matter, and in abundance sufficient to occasion consumption by its loss? and what are these disorders?"

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1806.

Second

Second prize. "What are the true characters and principal causes of that disorder which, though not confined to Poland, is nevertheless called *Plica Polonica?* Are there any methods of curing it with more success than by those hitherto known and employed? and what are those methods?" Prize questions.

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1807.

The third prize. "What are the principal maladies of vegetables, and what is the true analogy between those and the disorders of animals?"

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1808.

The class of Mathematical Societies of the same academy proposed the following prize:

"Suppose a canal from which flows per minute or second a quantity of water *m*. through a transverse section of given width and depth, terminated by two sides. This being admitted, if from one bank or side to the other there be constructed in the section a dyke or obstacle in which an aperture of given dimensions be made for the efflux of the water; it is demanded according to what law, the water elevated by means of the obstacle, will be forced to enlarge itself, not only near the dyke, but also in proceeding up the canal. It is desired that formulæ may be afforded sufficiently general to be applied to the efflux, not only of the same quantity m, but also any other quantity $m+x$. The theory and experiment, not being exactly corresponding with each other, it will be required that the necessary corrections should be made to the formulæ, and that it be proved by facts and observations how nearly they approach the truth."

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1806.

The class of Moral and Political Sciences have proposed as their first prize:

"As we see the mathematical and physical sciences make daily advances, and become enriched with new discoveries, it is demanded: 1st.—Why the same does not happen with regard to the moral sciences? 2d.—Among the different branches of these sciences, are there not some which are capable of greater perfection? and what are they? 3d.—To what point

are

Prize questions.

are they by their own nature capable of being advanced, and what are the limits which appear to bound their possible improvement? 4th.—What are the properest means for giving this possible degree of perfection to those parts of moral science? 5th.—It is more particularly desired that the discussion of this matter should be so conducted as to present results tending to advance the theory of the legislation most conformable to the nature of man."

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1806.

The second prize. "To determine by analytical investigation of political economy what are the points in which the fundamental positions of Adam Smith and Dr. Quesnay agree, and those in which they differ or are even opposite to each other. This examination must necessarily present results useful to the progress of the science of political economy."

The prize is 100 gold ducats of Holland; and the concurrence is open till the first of September 1806.

The dissertations are required to be written in Latin, French, or Polish, and the packet addressed to the rector of the Universaty of Vilna, to the care of Messrs. Layser or Karner, who are bankers in the same town, and to whom the rector will give a receipt. The University does not engage to return either the memoirs or drawings which shall be towarded in this competition; but the authors may take copies of the same at any time. These works will not be printed by the University without the the formal consent of the authors; but the authors themselves are at liberty to publish them in whatever manner they please.

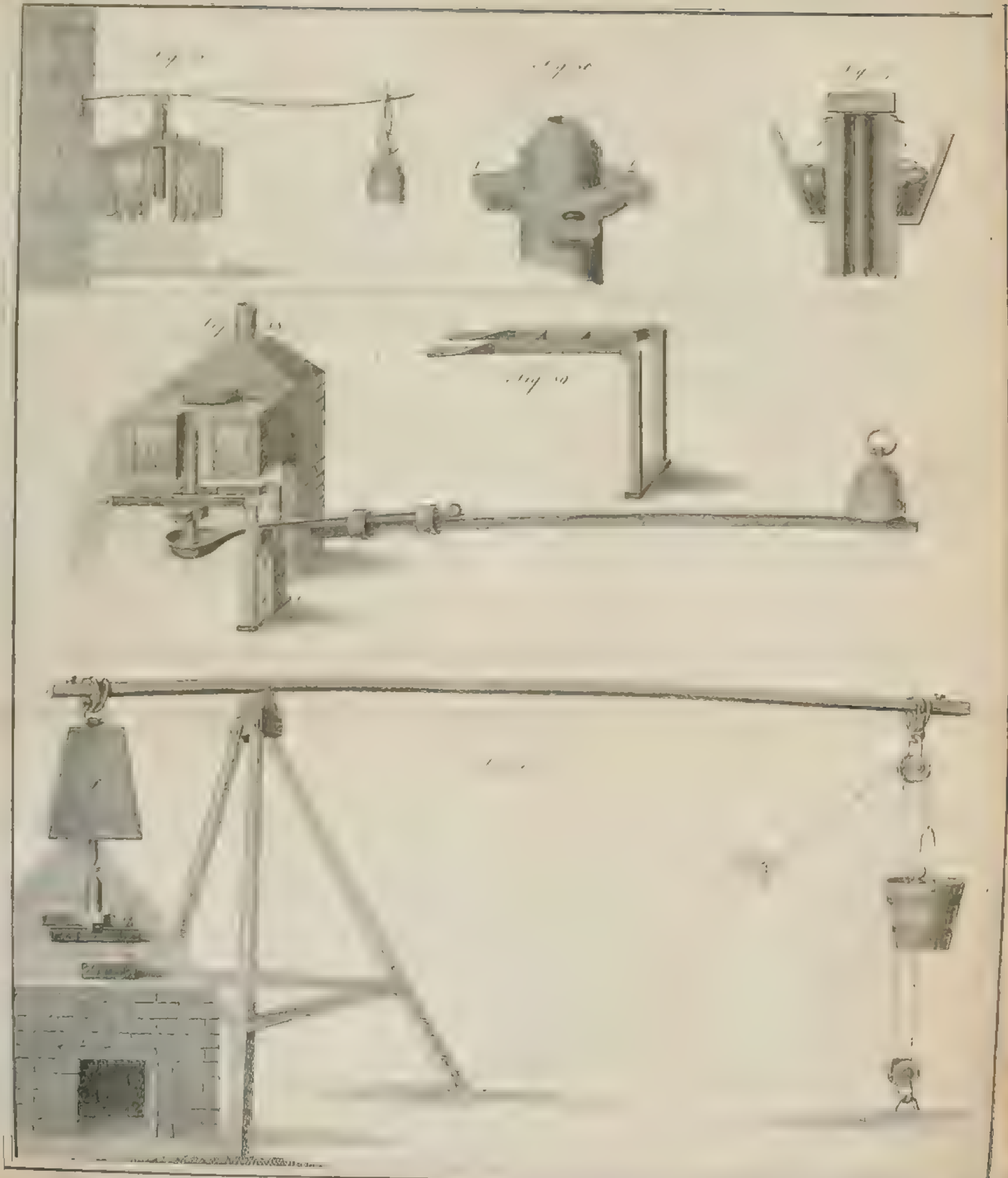
The prizes will be awarded before the new year, that is to say, before the first of January 1807, for these solutions made in the first year, and before the 1st of January 1808, for those made in the second year; and lastly, before the first of January 1809, for those made in the third year. The adjudications will be respectively announced in the public gazettes.

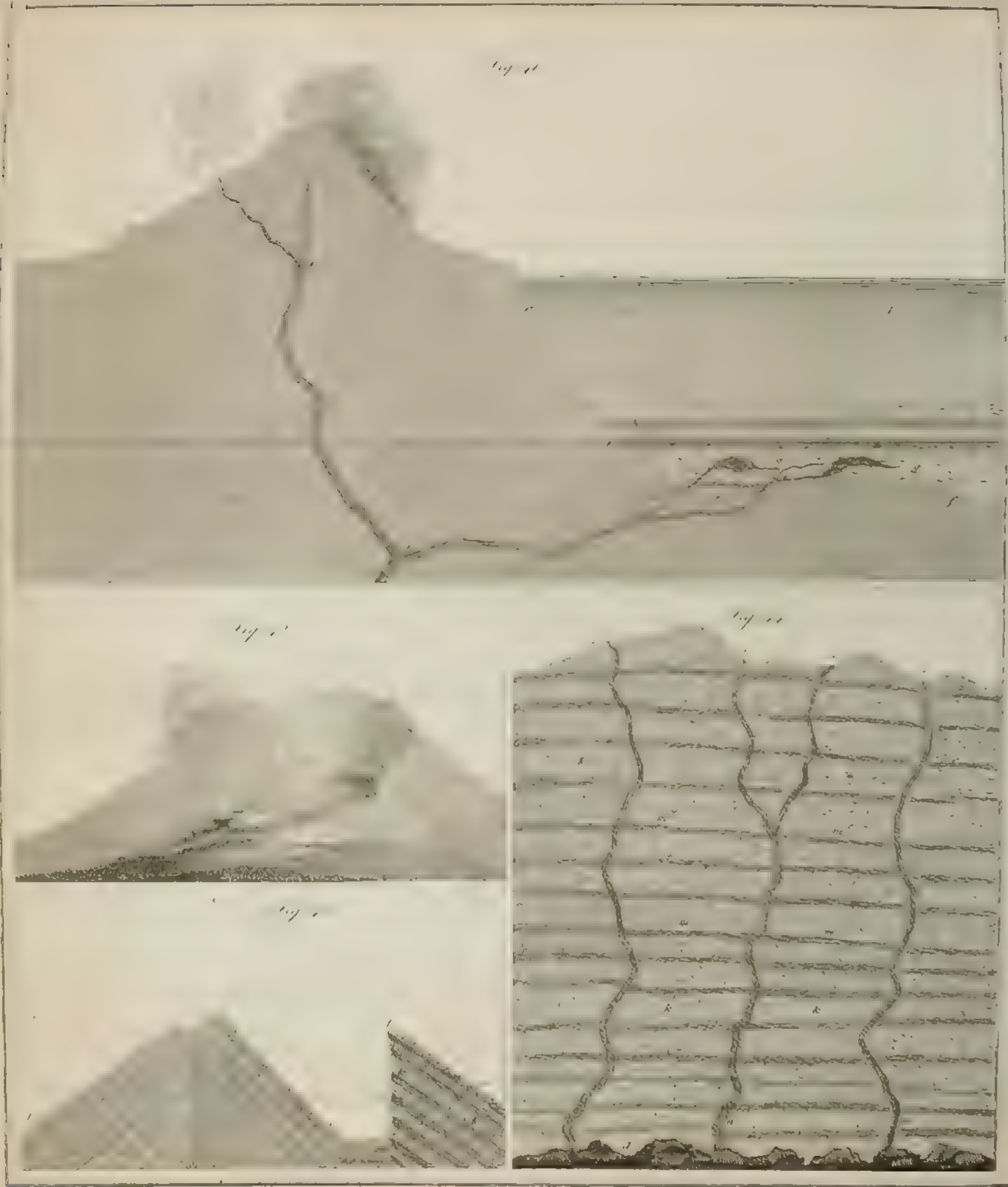
Each author may in person receive his prize from the administrative committee of the Imperial University of Vilna, or he may employ a person to whom he shall have given his full procuration. The prize will, according to the election of the candidate, consist either in the sum of money named, or a gold medal of the same value.

The actual professors and honorary members resident in Vilna are not admitted to this competition.

Engraved from Trans. R. S. Edin. Vol. 6. P. 193

PLATE IV.





A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1806.

ARTICLE I.

Letter from a Correspondent, enquiring the Cause why a Swell of the Sea is sometimes observed to precede a Storm from the same quarter. With some observations by the Editor.

To Mr. NICHOLSON.

SIR, *Helston, Cornwall, June 4, 1806.*

IT frequently happens on this coast that a heavy swell of the sea arrives from the westward, without any perceptible cause, which is followed by a gale of wind or storm from the same quarter many hours afterwards. I have observed the same fact on other coasts; and I believe the phenomenon is very generally known and admitted. It is not difficult to form a notion, that an expanded surface of water, undulating in a certain direction, may communicate a progressive motion to the air above it; but in all the theories relating to winds and waves, it has constantly, as far as my knowledge extends, been asserted that the waves are caused by the winds, and not the winds by the waves. We are also told by writers on meteorology,

An heavy swell often arrives on the Cornish coast, which is followed by a storm.

that a gentle breeze passes at the rate of about fifteen miles an hour; and that the velocity, if progressively augmented to sixty miles an hour, will produce a violent storm, of force sufficient to overthrow trees and houses. Now the velocity of the swell of the sea is so far from being in the least likely to produce any extreme progressive motion in the air, that I really think it never exceeds eight or ten miles an hour. Whatever you may think, Mr. Nicholson, of the importance of this philosophical difficulty, I hope you will have the goodness to propose it to your numerous correspondents. I have occasionally had the pleasure, in my constant perusal of your Journal, to remark that you have yourself sometimes given answers to questions which have been proposed. May I hope that you will not think mine undeserving your consideration.

Question: As the wind is not caused by the swell, but the contrary,—how happens it that the swell comes before the storm?

I am, Sir,

Your obliged reader,

M. M.

Reply. W. N.

Description of a squall at sea: It is a sudden and violent wind with rain.

I do not know that any of our philosophers have expressly considered the appearances which constitute what is called a squall at sea. A strong wind accompanied with rain comes on, almost instantaneously, and the impulse of this wind is sufficient to carry away a ship's topmasts, and even to do more material damage, if navigators were not to hold themselves in readiness to lower their sails as soon as the first impression takes place. The squall is more common in low latitudes than in high latitudes, where its duration is likewise shorter. It usually lasts eight or ten minutes or half an hour, and when it has ceased or passed to leeward, the ordinary wind, with which for the most part it coincides in direction, resumes its course.

Theory of winds. Much attention has been paid to horizontal currents of air, and but little to those streams which are directed upwards or downwards.

Every theory of the winds supposes part of the lower air to ascend, and that its place is supplied by an horizontal current. Very few writers have supposed a descending current to operate in the same way; and seldom indeed has any attention been paid to those direct or oblique ascending or descending winds which must be produced at the places where the causes of motion most powerfully operate.

I appre-

I apprehend the squall to be occasioned by a wind which blows immediately downwards. If we suppose a cloud to be suddenly condensed into drops (no matter by what process of chemistry, electricity, or other general cause, to us but little known), the falling drops or masses of water will occasion a descending current of air by their impulse against those portions of the atmosphere through which they pass. This kind of blast was formerly used in the water-blowing engine, an apparatus which acted with a force capable of supporting a pressure of about three feet of water.* But the tropical rains frequently descend with velocities much exceeding any that could be produced by engines of this kind, and their effects are much more striking.

The squall is a descending wind,

—produced by the impulse of falling rain.

It resembles the water-blowing engine.

The cloud which affords the descending stream is seldom stationary, but in general carried along by the common wind or lower current of the atmosphere; and the stream of descending water is certainly and in all cases affected with the same horizontal motion. The descending wind, which might otherwise have struck the water perpendicularly, is by this cause made to take an oblique direction, and runs rapidly along that surface, diverging from its principal place of descent in all directions, but most particularly in the direction which the wind already possesses. We must therefore be careful to distinguish two kinds of velocity belonging to the squall or descending current; namely, first, the velocity of the current itself, which is greatest at the place of descent, and diminishes in receding from that place; and, secondly, the velocity of the cloud, or blowing apparatus, which is carried along by the common horizontal current or prevailing wind. The first of these two velocities constitutes, within its limitted sphere of activity, a storm; and the other velocity measures the progress with which the storm is horizontally conveyed along. Let us suppose a stone to be thrown into smooth water; its action will cause a wave to be propagated horizontally to great distances around: In the same manner we can conceive a wave or a swell to be raised by the impulse of descending air, and propagated around in all directions, though modified by the winds it may meet. This swell, moving uniformly, will not be propagated as speedily as the descending current, where this last is most rapid; but

The cloud, which affords the blast, moves horizontally, and therefore gives an oblique impulse to the sea.

The velocity of the blast and that of the clouds are distinct objects;

—the first produces a local storm, and the second carries the storm forwards along the sea.

* Lewis, in his Philos. Commerce of Arts.

at a certain distance from the place of greatest storm the swell will proceed, modified only by the common wind which it will either outrun, or follow, or cross, according to circumstances,—nothing being more common at sea than to have the wind in one direction and the swell in another.

All storms are probably descending currents of wind, or extensive squalls.

I strongly suspect that a squall is a storm in miniature, or that all storms are produced by descending streams from the upper part of the atmosphere, caused, in some instances, by the fall of great masses of water, and in others by chemical processes, concerning which we scarcely dare to form a conjecture.

Uncommon squalls or short storms. White squall; typhon; showers of stones; narrow streams of air, &c.

The white squall, or squall without rain, in the Chinese seas; the typhon, or storm of twelve or eighteen hours, which, in the same regions, comes on suddenly, and blows with extreme violence, in succession from almost every point of the compass; the sudden condensation or production of ignited stones, which have so often fallen with great agitation of the atmosphere; the limited streams of air which have been known to rush across the face of a country, making a narrow line of devastation;—these, and many other facts of whirlwinds, waterspouts, explosive noises, and the like, shew that the air may be put into violent motion by other causes as well as its change of elasticity from heat and cold, and the mechanical action of descending water.

Concluding remarks. The swell, caused by a storm, may be propagated with a greater mean velocity than the storm that causes it, and may therefore arrive on a coast before it,

Whatever great and powerful agent may therefore cause the descending air to throw the sea into a swell, it does not necessarily follow that the swell shall exist only in the vicinity of the blast which occasioned it. The centre of action, if it may be so called, may either be stationary, or it may move along with any determinate degree of velocity. From the nature of the case, its motion will be such as to follow or coincide with that of the swell it causes. Whenever a heavy swell arrives upon any coast, it will, according to the doctrine here laid down, indicate that a storm or long continued squall has existed, and probably continues to exist, towards that point of the compass from which the swell arrives. If its progressive velocity and duration be sufficient, the storm will arrive at the coast subsequent to the swell, unless its first generation or commencement was near the shore; but we may suppose, and undoubtedly it often so happens, that the atmospheric cause of the swell may have ceased long before the swell itself has subsided.

—or come after the storm has ceased.

II.

II.

Enquires respecting various subjects relating to the Arts. By JUVENIS. *With some remarks by* W. N.

To Mr. NICHOLSON.

SIR,

YOUR kind attention to your correspondent R. B. vol. vii. 71, has induced me to trouble you with the following queries, not doubting but that from you or your learned correspondents, I shall receive a satisfactory answer to them.

Query 1.—Respecting the incrustation left by water upon iron boilers.

1st. Is there any method of freeing iron boilers from the incrustation left on them by hard water, without subjecting them to a red heat. Copper vessels, I know, are readily so cleaned, but cast iron will not stand that heat without cracking.

Query 2.—On the taking off counterproofs from engravings.

2d. Is it possible to reduce printing ink to a fluid state, so as to take a copy of a copper-plate, or printed paper, similar to the method used in copying writing, and this without the original receiving any other damage than that of becoming fainter, which the loss of the ink must inevitably occasion.

Query 3.—Imperfections of speculums.

3d. Is there any method of ascertaining the degree of porosity of the metals of reflecting telescopes, except by their performance when polished? Edwards says the porosity arises from the calcination of the tin, and I have tried a number of ways to prevent this, but am at a loss which to prefer, for want of some criterion whereby to judge of the degree of porosity in each separate metal. When the tin is left long in the fire, the metal I know will be bad, but I cannot (when well polished), even with the highest magnifiers, discover any pores in it, though I have no doubt but the badness of its performance arises from that cause. But the performance alone cannot certainly indicate the degree of porosity, since I find metals of the same composition, when ground and polished in every respect the same, seldom or never exactly agree in performance.

Query 4.—Supposed achromatic eye-glass.

4th. In a late edition of Ferguson's lectures by Brewster, the following rule is given for making the eye-pieces of telescopes achromatic. With two lenses, the focal length of the first three times

times that of the second, and their distance $\frac{2}{3}$ the focal length of the first. I must own I do not see the reason why this combination should remove the chromatic aberration, nor does the author give any. He hints also how much superior such combinations of lenses are for magnifiers of solar microscopes; if this is the case, why will not a similar combination answer for the object-glass of a telescope?

That a combination of lenses may diminish or nearly remove the error arising from the spherical figure, I can easily conceive, but not the chromatic aberration.

JUVENIS.

June 2d, 1806.

The above questions may, and to you I dare say will, appear trifling and frivolous; but you will pardon them when you reflect how frequently some little obstacle starts up to stop the course of the young and ardent enquirer, and that unless a kind friend remove it, seems an insurmountable barrier.

Reply.

Singular fact respecting the incrustation in a kettle.

I wish it were in my power to give full information concerning the objects of the present letter. What it may be that incrusts the surface of tea-kettles, and is very troublesome in steam engines, I do not know, but suppose it to be sulphate of lime. Many years ago I was informed, that if an oyster-shell be constantly kept in a tea-kettle there will be no incrustation formed, except upon the shell. If this be true, the same effect would be produced by a loose piece of incrustation, provided it were put in before any deposition had began to fix upon the metal.

Porosity of speculum metal.

2. It does not seem probable that a bad speculum which has no pores discernible by the microscope owes its badness to porosity. When I was formerly busied upon speculums, it was my custom to grind off a small portion of the metal and polish it, in order to determine whether I should bestow an farther labour upon the piece. This polish was often, for the sake of expedition, given with a leather or buff stick; and it always happened that the surface took a wavy appearance; an effect which did not follow when pitch was used as the polisher. I ascribed this irregularity to some parts being softer than

Speculum metal appears wavy if polished on leather.

than others, and on that account yielding more to the pressure of the springy leather; and the differences of hardness seemed to me to have arisen from the nature of the crystallization in cooling. The slower the cooling the larger will be the crystals and the coarser the grain upon fracture. I did not pursue this object far; but I found, and I believe it is general to all castings, that the colder the metal, when poured out, the denser specifically and more uniform in its fracture was the cast. These facts and observations seem to give probability to the following inference: that if two metals were cast by successive pouring out of the same pot, the latter would have more density and uniformity of aggregation and hardness, and would take a better figure upon the polisher, which though of pitch, and supposed to have no elasticity, has, it is most likely, enough of spring to affect a figure which would be essentially injured by a deviation of one millionth part of an inch.*

Cause of this effect supposed to lie in the crystallization in cooling.

It is thought that a cast of metal nearly cold would take the best figure.

3. I do not see how chromatic abberration is to be corrected by refraction towards the same parts, unless by mediums, such as described by Doctor Blair in his paper on aplanatic refraction, in the second volume of the Edinburgh Transactions, of which an abridgment is given in our Quarto Series, vol. i. p. 1. But these do not apply to the case mentioned by Mr. Brewster.

On achromatic eye-pieces.

III.

On the means of obtaining Stamps (Clichés) with Moulds of Plaster of Paris, Sulphur, or Sealingwax. By M. DARCET. *Read at the Society of Encouragement, the* 12*th of February,* 1806.†

FOUNDERS use the term *clicher* for making impressions on metals without casting them in a mould; and that of

Art of stamping, or making impressions in metals.

* Without recurring to optical principles, as to the figure of a speculum, it may be observed that a single stroke on the polisher will sensibly alter that figure; but that a great number of strokes would be required to work out a scratch of one hundred thousand to the inch in breadth.

† Translated from *La Revue Philosophique, Litteraire, et Politique,* No. 10, for April, 1806, p. 1.

cliché

cliché for an impression thus made. The fabrication of *assignats*, or rather the lamentable necessity of reproducing them without end, was in some degree the creating cause of the art of taking metallic impressions (*de clicher*), which does not seem at the first view to be of any great importance; but it is in reality capable of being applied with advantage to several arts, and particularly to those connected with the arts of design.

Improved by M. Darcet.

M. Darcet, the son of the celebrated chemist, in forming a collection of impressions from engraved gems, has just carried this art to a great extent. From an object of amusement, for which he at first began to employ it, he has applied it to the manufactory of paper-hangings, and even of printed cottons, of ornamental furniture, of artificial flowers, and in short of every thing that taste or industry would decorate. It is easy to conceive how much expence may be saved by taking impressions of engravings already executed instead of making new ones.

Its uses.

The following is an account of the advantages, which Mr. Darcet expects from his process, as exhibited in the accurate report delivered by him to the society.

For blocks for paper-hangings and printed goods.

"In the manufacture of paper-hangings and printed goods, the pattern is printed by means of wooden blocks; and these, though cut in a coarse manner and at a moderate price, appeared to me capable of having their place in part supplied by the processes of metallic impression *(clichage)*. My trials were successful; and the various specimens laid before the assembly, show that this art may be carried to a great extent, and cannot fail to produce beneficial results if pursued by expert hands: in fact, all that is requisite is to model in wax or plaster of Paris the ornament we would multiply, and to obtain from it as many matrices, and consequently as many metallic stamps as the block is required to contain.

Capable of forming a number of patterns by varying the combination of the same figures.

"This process is particularly applicable to blocks in which the same ornament is repeated several times. It has likewise the advantage of furnishing very speedily a great number of separate pieces, which may afterward be combined in a great variety of ways, to form at a very trifling expence, a number of patterns differing from each other.

In printing calicos the mordants may

"I am aware, that in many cases it will be more advantageous to cut wooden blocks for paper-hangings; and I apprehend

hend that the mordants employed for printed goods may frequently render the use of metallic blocks unadvisable: but a few inconveniences should not lead us to reject a process which may speedily be brought to perfection, as we are led to expect from the purposes to which it has already been applied, and the success obtained. sometimes be an obstacle to the use of this art.

"The upholsterer will ornament his goods at a small expence, by multiplying the engraved gems and basso relievoes of antiquity, and the fine productions of modern art, by means of plaster moulds. In furniture.

"These ornaments will be in a style as chaste as that of the models, of which they will only be polytypes; and the labours of the carver and sculptor becoming needless, works may be afforded at a low price, of such a degree of perfection, as could not by any other means be attained without considerable charge. A cheap substitute for carving.

"The maker of artificial flowers, by employing plaster of Paris, may find a cheap method of procuring solid moulds taken from the leaves themselves, and more perfect than those imitations made by art. In artificial flower making.

"The figurer of stuffs too may substitute moulds of metal for those of plaster of Paris, sulphur, and wood, which he commonly employs. In figuring stuffs.

"The antiquary may use metallic impressions instead of the plaster casts and sulphurs of his cabinet; and his impressions, thus rendered more solid, will have the advantage of being capable of being multiplied more easily without fear of injuring them, and his collection will be increased by the exchange of copies he may make with others. In imitating ancient coins and gems.

"Lastly, this art affords the means of improving engraving in relief; of which I have already exhibited proofs, and more positive demonstration will soon be afforded the Society. I hope it will see brought to perfection, an art almost lost among us, the success and advantages of which, however, concern many branches of national industry. For engraving in relief, or block cutting.

"I might mention several other uses of stamps or blocks made with plaster of Paris *(du clichage en plâtre)*, and I conceive there are several cases in which it may be of utility, particularly when the process shall have attained all the perfection of which it is susceptible." Blocks of plaster.

The whole report of M. Darcet may be read in the *Bulletin de la Société d' Encouragement*, No. 20, Feb. 1806. It is not a simple process, but an art described in a clear and simple manner, with all its circumstances, such as we could wish the description of every art to be given.

Principal points to be attended to.

The two principal points are the preparation of the moulds, and the metal intended for stamps. Though good stamps may be obtained by letting a perfectly dry plaster mould fall on the fusible compound of bismuth, tin, and lead, M. Darcet has thought that, if he could impart more solidity to the plaster, and fill up the pores that form bubbles or diminish the polish of the stamp, he should improve the art very much. After having tried several means he has preferred Flanders glue.

Method of making the plaster moulds. They are steeped in weak hot glue.

"Three ounces and half avoirdupois weight of fine Flanders glue are to be steeped, and then dissolved by heat in four pints and half of water. The solution being strained through a piece of fine linen or a sieve, is to be heated till it is near boiling, and the plaster moulds, previously dried and slightly heated, are then to be immersed in it. The air they contain expands and escapes, while the water, taking its place, carries with it into the interior parts of the mould the glue which it held in a state of considerable attenuation. As soon as the air ceases to be disengaged, the plaster mould is to be taken out and shaken, and the operator must blow strongly on its engraved surface, that pellicles of glue may not be formed in cooling, which would impair the delicacy of the work."

The moulds thus saturated should be dried slowly; toward the end, however, the temperature may be raised as high as 50° or 60° of the centigrade thermometer (112° or 140° Farht.)

The moulds must be kept dry, and used dry and cold.

The moulds thus saturated with glue must not be used except when very dry; and as they attract the moisture of the air, in consequence of the glue contained in them, they must be kept in a dry place, or heated before they are used, taking care to let them grow cold before the metal is stamped with them.

Darcet's fusible metal,

As to the metallic compound, it was invented by M. Darcet, senior. That worthy man, who found no pleasure in his discoveries without rendering them public and useful, made this known in the years 1773 and 1777, by means of the *Journal de Médecine* and the *Journal de Physique*. It has since been employed

employed for the fabrication of assignats, and may be esteemed the foundation of the stereotype art. M. Erhmann will testify this. —used in fabricating assignats, and the foundation of stereotypes.

In making stamps with moulds of plaster, the success depends greatly on the fusibility of the metal; that employed by M. Darcet is the most fusible known. The most fusible known.

"Numerous experiments," says M. Darcet, "convinced my father, that the compound should be formed of eight parts of bismuth, five of lead, and three of tin. When it is well made, it begins to soften at 91° of the centigrade thermometer (196° Farht.), and melts between 92° and 93° (about 199° Farht.). 8 parts bismuth, 5 lead, 3 tin, melts 13 degrees below boiling water.

"This compound when cold is sufficiently malleable to stand a blow, and hard enough to preserve the impressions from friction, and to allow them to be retouched with the graver. Sufficiently hard when cold.

"The following is the best mode of preparing it:

"Fuse the bismuth, cover it with resin or suet, and heat the whole rather strongly; add the lead, stir them well together, increase the temperature a little, and add to the metals in fusion the requisite quantity of tin. Stir the mixture again, and cast it into a plate or ingot. Mode of preparing it.

"This compound is to be used only in the soft state, that is, at a degree of heat much below the point of boiling water; it is then fitted for receiving the impression of the matrix, without emitting the air and water this contains, which would form blebs or flaws in the stamp. To be used only when soft.

"The soft state is that which the fusible metal assumes at 91° (196° Farht.). When it begins to grow solid, a crystallization forms in the still fluid mass of metal; and this crystallization must be broken and rendered confused, by agitating the metal as quickly as possible, kneading it as it were, and particularly by bringing the part near the edges to the centre, and alternately carrying the central part to the edges. Directions for using it.

"When the whole of the metal is reduced to this state, nothing more is necessary, but to stamp it quickly with the plaster matrix.

"Every time this compound is fused, oxided pellicles are formed on its surface; and the greater heat, or the longer it is continued, the more considerable they are. These scoriæ should be collected together, and afterwards fused with resin, Scoriæ,

 oil,

to be removed. oil, or tallow, at a temperature sufficiently high to reduce them to the metallic state, so that they may be used in subsequent operations."

The manipulation easy. Nothing more remains but the mechanical process, which is easy, and for which we must refer to the Bulletin of the *Société d' Encouragement* already quoted. This Bulletin should be consulted by all who would know the minutiæ of the art. Fidelity of the process. We shall conclude by observing, that the fidelity of the impressions or stamps *(clichés)* is so great, as to give with the greatest accuracy every ramification in a rose leaf.

During the fabrication of assignats, they were stamped both by the hand and by engines. Like all other useful processes, this was soon improved in consequence of the necessity of repeating it. The operation is rendered more expeditious and The stamping performed by a simple machine. more exact by means of a simple machine. A plate added to the paper of M. Darcet, and engraved in the Bulletin of the *Société d' Encouragement*, represents this machine which is described in M. Darcet's paper.

To this abstract were subjoined some impressions taken with plaster of Paris from wooden cuts of M. Duplat, and then worked off from stamps made with M. Darcet's compound metal.*

IV.

Account of a series of Experiments, shewing the Effects of Compression in modifying the Effects of Heat. By Sir James Hall, *Bart. F. R. S. Edinburgh.*

(Continued from page 128.)

Sect. viii.—*Formation of Coal.—Accidental occurrence which led me to undertake these Experiments.—Results extracted from a former publication.—Explanation of some*

* The prints in *La Revue* exactly resemble wood-cuts. The press-work not being performed in a better manner than common, it is not possible to form any judgment of the minute differences.

diffi-

difficulties that have been suggested.—The Fibres of Wood in some cases obliterated, and in some preserved under compression.—Resemblance which these Results bear to a series of Natural Substances described by Mr. Hatchett.—These results seem to throw light on the history of Surturbrand.

As I intend, on some future occasion, to resume my experiments with inflammable substances, which I look upon as far from complete, I shall add but a few observations to what I have already laid before this Society, in the sketch I had the honour to read in this place on the 30th of August last.

Inflammable substances.

The following incidental occurrence led me to enter upon this subject rather prematurely, since I had determined first to satisfy myself with regard to the carbonate of lime.

Observing, in many of the last-mentioned class of experiments, that the elastic matters made their escape between the muzzle of the barrel and the cylinder of lead, I was in the habit, as mentioned above, of placing a piece of leather between the lead and the barrel; in which position, the heat to which the leather was exposed, was necessarily below that of melting lead. In an experiment, made on the 28th of November 1803, in order to ascertain the power of the machinery, and the quantity of metal driven out by the expansion of the liquid, there being nothing in the barrel but metal, I observed, as soon as the compressing apparatus was removed, (which on this occasion was done while the lower part of the barrel was at its full heat, and the barrel standing brim full of liquid metal), that all the leather which lay on the outside of the circular muzzle of the barrel, remained, being only a little browned and crumpled by the heat to which it had been exposed. What leather lay within the circle, had disappeared; and on the surface of the liquid metal, which stood up to the lip of the barrel, I saw large drops of a shining black liquid, which, on cooling, fixed into a crisp black substance, with a shining fracture, exactly like pitch or pure coal. It burned, though not with flame. While hot, it smelt decidedly of volatile alkali. The important circumstance here, is the different manner in which the heat had acted on the leather, without and within the rim of the barrel. The only difference consisted in compression, to which, therefore, the difference of effect must be ascribed: by its force, the volatile matter

Incidental occurrence wherein leather was exposed to heat under compression,

—and converted into coaly matter.

matter of the leather which escaped from the outward parts, had within the rim, been constrained to remain united to the rest of the composition, upon which it had acted as a flux, and the whole together had entered into a liquid state, in a very low heat. Had the pressure been continued till all was cool, these substances must have been retained, producing a real coal.

Direct experiments.

On the 24th April 1803, a piece of leather used in a similar manner, (the compressing force being continued, however, till all was cold,) was changed to a substance like glue, owing doubtless to compression in a heat under that of melting lead.

These observations led me to make a series of experiments with animal and vegetable substances, and with coal; the result of which I have already laid before the Society. I shall now repeat that communication, as printed in Nicholson's Journal for October last (1804.)*

Pit coal exposed to heat under compression.

"I have likewise made some experiments with coal, treated in the same manner as the carbonate of lime: but I have found it much less tractable; for the bitumen, when heat is applied to it, tends to escape by its simple elasticity, whereas the carbonic acid in marble, is in part retained by the chemical force of quicklime. I succeeded, however, in constraining the bituminous matter of the coal, to a certain degree, in red-heats, so as to bring the substance into a complete fusion, and to retain its faculty of burning with flame. But, I could not accomplish this in heats capable of agglutinating the carbonate; for I have found, where I rammed them successively into the same tube, and where the vessel has withstood the expansive force, that the carbonate has been agglutinated into a good limestone, but that the coal has lost about half its weight, together with its power of giving flame when burnt, remaining in a very compact state, with a shining fracture. Although this experiment has not afforded the desired result, it answers another purpose admirably well. It is known, that where a bed of coal is crossed by a dike of whinstone, the coal is found in a peculiar state in the immediate neighbourhood of the whin: the sub-

When some of the volatile matter escaped a production

* As the present extract is short, it was thought better to repeat it here, than to interrupt the subject by referring to our ixth volume. Ed.

stance

stance in such places being incapable of giving flame, it is distinguished by the name of *blind coal.* Dr. Hutton has explained this fact, by supposing that the bituminous matter of the coal, has been driven by the local heat of whin, into places of less intensity, where it would probably be retained by distillation. Yet the whole must have been carried on under the action of a pressure capable of constraining the carbonic acid of the calcareous spar, which occurs frequently in such rocks. In the last-mentioned experiment, we have a perfect representation of the natural fact; since the coal has lost its petroleum, while the chalk in contact with it has retained its carbonic acid.

resembling blind coal was afforded.

"I have made some experiments of the same kind, with vegetable and animal substances. I found their volatility much greater than that of coal, and I was compelled, with them, to work in heats below redness; for, even in the lowest red-heat, they were apt to destroy the apparatus. The animal substance I commonly used was horn, and the vegetable, saw-dust of fir. The horn was incomparably the most fusible and volatile of the two. In a very slight heat, it was converted into a yellow-red substance, like oil, which penetrated the clay tubes through and through. In these experiments, I therefore made use of tubes of glass. It was only after a considerable portion of the substance had been separated from the mass, that the remainder assumed the clear black peculiar to coal. In this way I obtained coal, both from saw-dust and from horn, which yielded a bright flame in burning.

Animal and vegetable matters treated in the same manner.

"The mixture of the two produced a substance having exactly the smell of soot or coal-tar. I am therefore strongly inclined to believe, that animal substance, as well as vegetable, has contributed towards the formation of our bituminous strata. This seems to confirm an opinion, advanced by Mr. Keir, which has been mentioned to me since I made this experiment. I conceive, that the coal which now remains in the world, is but a small portion of the organic matter originally deposited: the most volatile parts have been driven off by the action of heat, before the temperature had risen high enough to bring the surrounding substance into fusion, so as to confine the elastic fluids, and subject them to compression.

It is probable that animal as well as vegetable matter has contributed to our bituminous strata.

"In

Animal matter totally dissipated under compression, which would else have left a coak.

"In several of these experiments, I found, when the pressure was not great, when equal, for instance, only to 80 atmospheres, that the horn employed was dissipated entirely, the glass tube which had contained it being left almost clean: yet undoubtedly, if exposed to heat without compression, and protected from the contact of the atmosphere, the horn would leave a cinder or coak behind it, of matter wholly devoid of volatility. Here, then, it would seem as if the moderate pressure, by keeping the elements of the substance together, had promoted the general volatility, without being strong enough to resist that expansive force, and thus, that the whole had escaped. This result, which I should certainly not have foreseen in theory, may perhaps account for the absence of coal in situations where its presence might be expected on principles of general analogy."

Since this publication, a very natural question has been put to me. When the inflammable substance has lost weight, or when the whole has been dissipated, in these experiments, what has become of the matter thus driven off?

It is uncertain what became of the matters volatilized under pressure.

I must own, that to answer this question with perfect confidence, more experiments are required. But, in the course of practice, two circumstances have occurred as likely, in most cases, to have occasioned the loss alluded to. I found in these experiments, particularly with horn, that the chalk, both in powder and in lump, which was used to fill vacuities in the tubes, and to fix them in the cradle, was strongly impregnated with an oily or bituminous matter, giving to the substance the qualities of a stinkstone. I conceive, that the most volatile part of the horn has been conveyed to the chalk, partly in a state of vapour, and partly by boiling over the lips of the glass tube; the whole having been evidently in a state of very thin fluidity. Having, in some cases, found the tube, which had been introduced full of horn, entirely empty after the experiment, I was induced, as above stated, to conceive, that, under pressure, it had acquired a greater general volatility than it had in freedom; and I find that, in the open fire, horn yields a charcoal equal to 20 *per cent.* of the original weight. But more experiments must be made on this subject.

Another

Another cause of the loss of weight, lay undoubtedly in the excess of heat employed in most of them, to remove the cradle from the barrel. With inflammable substances, no air-tube was used, and the heats being low, the air lodged in interstices had been sufficient to secure the barrels from destruction, by the expansion of the liquid metal. In this view, likewise, I often used lead, whose expansion in such low heats, I expected to be less than that of the fusible metal. And the lead requiring to melt it, a heat very near to that of redness, the subject of experiment was thus, on removing the cradle, exposed in freedom to a temperature which was comparatively high. But, observing that a great loss was thus occasioned, I returned to the use of the fusible metal, together with my former method of melting it, by plunging the barrel, when removed from the furnace, into a solution of muriate of lime, by which it could only receive a heat of 250° of Fahrenheit.

Some part was lost in taking the subject out of the apparatus.

The effect was remarkable, in the few experiments tried in this way. The horn did not, as in the other experiments, change to a hard black substance, but acquired a semifluid and viscid consistency, with a yellow-red colour, and a very offensive smell. This shews, that the substances which here occasioned both the colour and smell of the results, had been driven off in the other experiments, by the too great heat applied to the substance, when free from compression.

Observation.

I found that the organization of animal substance was entirely obliterated by a slight action of heat, but that a stronger heat was required to perform the entire fusion of vegetable matter. This, however, was accomplished; and in several experiments, pieces of wood were changed to a jet-black and inflammable substance, generally very porous, in which no trace could be discovered of the original organization. In others, the vegetable fibres were still visible, and are forced asunder by large and shining air-bubbles.

Slight heat destroys the organization of animal matter; vegetable requires strong heat.

Since the publication of the sketch of my experiments, I have had the pleasure to read Mr. Hatchett's very interesting account of various natural substances, nearly allied to coal; and I could not help being struck with the resemblance which my results bear to them, through all their varieties, as brought into view by that able chemist; that resemblance affording a pre-

Substances mentioned by Mr. Hatchett and elucidated by these experiments.

presumption, that the changes which, with true scientific modesty, he ascribes to an unknown cause, may have resulted from various heats acting under pressure of various force. The substance to which he has given the name of *Retinasphaltum*, seems to agree very nearly with what I have obtained from animal substance, when the barrel was opened by means of low heat. And the specimen of wood entering into fusion, but still retaining the form of its fibres, seems very similar to the intermediate substance of Bovey-coal and *Surturbrand*, which Mr. Hatchett has assimilated to each other. It is well known, that the surturbrand of Iceland, consists of the stems of large trees, flattened to thin plates, by some operation of nature hitherto unexplained. But the last-mentioned experiment seems to afford a plausible solution of this puzzling phenomenon.

Application of the author's experiments to other natural events.

In all parts of the globe, we find proofs of slips, and various relative motions, having taken place amongst great masses of rock, whilst they were soft in a certain degree, and which have left unequivocal traces behind them, both in the derangements of the beds of strata, and in a smooth and shining surface, called *slickenside*, produced by the direct friction of one mass on another. During the action of subterranean heat, were a single stratum to occur, containing trees intermixed with animal substances, shell-fish, &c. these trees would be reduced, to a soft and unctuous state, similar to that of the piece of wood in the last-mentioned experiment, whilst the substance of the contiguous strata retained a considerable degree of firmness. In this state of things, the stratum just mentioned, would very naturally become the scene of a slip, occasioned by the unequal pressure of the surrounding masses. By such a sliding motion, accompanied by great compression, a tree would be flattened, as any substance is ground in a mortar, by the combination of a lateral and direct force. At the same time, the shells along with the trees, would be flattened, like those described by Bergman; while those of the same species in the neighbouring limestone-rock, being protected by its inferior fusibility, would [illegible] their natural shape.

SECT.

SECT. IX.—*Application of the foregoing results to Geology.—The fire employed in the Huttonian Theory is a modification of that of the Volcanoes.—This modification must take place in a lava previous to its eruption.—An Internal Lava is capable of melting Limestone.—The effects of Volcanic Fire on substances in a subterranean and submarine situation, are the same as those ascribed to Fire in the Huttonian Theory.—Our Strata were once in a similar situation, and then underwent the action of fire.—All the conditions of the Huttonian Theory being thus combined, the formation of all Rocks may be accounted for in a satisfactory manner.—Conclusion.*

Having investigated, by means of the foregoing experiments, some of the chemical suppositions involved in the Huttonian Theory, and having endeavoured to assign a determinate limit to the power of the agents employed; I shall now apply these results to Geology, and inquire how far the events supposed anciently to have taken place, accord with the existing state of our globe.

The results applied to geology.

The most powerful and essential agent of the Huttonian Theory, is Fire, which I have always looked upon as the same with that of volcanoes, modified by circumstances which must, to a certain degree, take place in every lava previous to its eruption.

Fire is the essential agent of the Huttonian theory.

The original source of internal fire is involved in great obscurity; and no sufficient reason occurs to me for deciding whether it proceeds by emanation from some vast central reservoir, or is generated by the local operation of some chemical process. Nor is there any necessity for such a decision: all we need to know is, that internal fire exists, which no one can doubt, who believes in the eruptions of Mount Vesuvius. To require that a man should account for the generation of internal fire, before he is allowed to employ it in geology, is no less absurd than it would be to prevent him from reasoning about the construction of a telescope, till he could explain the nature of the sun, or account for the generation of light.*

To account for the origin of that fire is no part of the doctrine. It is considered as a mere fact.

* This topic, however, has of late been much urged against us, and an unfair advantage has been taken of what Mr. Playfair has said upon it. What he gave as mere conjecture on a subject of collateral importance, has been argued upon as the basis and fundamental doctrine of the system.

 But

But while we remain in suspense as to the prime cause of this tremendous agent, many circumstances of importance with regard to it, may fairly become the subjects of observation and discussion.

Volcanic fire is deep seated,

Some authors (I conceive through ignorance of the facts) have alleged, that the fire of Ætna and Vesuvius is merely superficial. But the depth of its action is sufficiently proved, by the great distance to which the eruptive percussions are felt, and still more, by the substances thrown out uninjured by some eruptions of Mount Vesuvius. Some of these, as marble and gypsum, are incapable in freedom of resiting the action of fire. We have likewise granite, schistus, gneiss, and stones of every known class, besides many which have never, on any other occasion, been found at the surface of our globe. The circumstance of these substances having been thrown out, unaffected by the fire, proves, that it has proceeded from a source, not only as deep, but deeper, than their native beds; and as they exhibit specimens of every class of minerals, the formation of which we pretend to explain, we need inquire no further into the depth of the Vesuvian fire, which has thus been proved to reach below the range of our speculations.

—and subject to perpetual and irregular variations.

Volcanic fire is subject to perpetual and irregular variations of intensity, and to sudden and violent renewal, after long periods of absolute cessation. These variations and intermissions, are likewise essential attributes of fire as employed by Dr. Hutton; for some geological scenes prove, that the indurating cause has acted repeatedly on the same substance, and that, during the intervals of that action, it had ceased entirely. This circumstance affords a complete answer to an argument lately urged against the Huttonian Theory, founded on the waste of heat which must have taken place, as it is alleged, through the surface. For if, after absolute cessation, a power of renewal exists in nature, the idea of waste by continuance is quite inapplicable.

The external phenomena of volcanoes are sufficiently well known; but our subject leads us to inquire into their internal actions. This we are enabled to do by means of the foregoing experiments, in so far as the carbonate of lime is concerned.

Some

Some experiments which I formerly * laid before this Society and the public, combined with those mentioned in this paper, prove, that the feeblest exertions of volcanic fire, are of sufficient intensity to perform the agglutination, and even the entire fusion, of the carbonate of lime, when its carbonic acid is effectually confined by pressure; for though lava, after its fusion, may be made, in our experiments, to congeal into a glass, in a temperature of 16° or 18° of Wedgwood, in which temperature the carbonate would scarcely be affected; it must be observed, that a similar congelation is not to be looked for in nature; for the mass, even of the smallest stream of lava, is too great to admit of such rapid cooling. And, in fact, the external part of a lava is not vitreous, but consists of a substance which, as my experiments have proved, must have been congealed in a heat of melting silver, that is, in 22° of Wedgwood; while its internal parts bear a character indicating that they congealed in 27° or 28° of the same scale. It follows, that no part of the lava, while it remained liquid, can have been less hot than 22° of Wedgwood. Now, this happens to be a heat, in which I have accomplished the entire fusion of the carbonate of lime, under pressure. We must therefore conclude, that the heat of a running lava is always of sufficient intensity to perform the fusion of limestone.

A low volcanic heat will fuse the carbonate of lime.

Lavas, congealed between 22° and 28°.

In every active volcano, a communication must exist between the summit of the mountain and the unexplored region, far below its base, where the lava has been melted, and whence it has been propelled upwards; the liquid lava rising through this internal channel, so as to fill the crater to the brim, and flow over it. On this occasion, the sides of the mountain must undergo a violent hydrostatical pressure outwards, to which they often yield by the formation of a vast rent, through which the lava is discharged in a lateral eruption, and flows in a continued stream sometimes during months. On Ætna most of the eruptions are so performed; few lavas flowing from the summit, but generally breaking out laterally, at very elevated stations. At the place of delivery, a quantity of gaseous matter is propelled violently upwards, and, along with it, some liquid lava; which last, falling back again in a spongy state, produces one of those conical hills which we see

The phenomena of active volcanoes.

* *Edinburgh Transactions*, vol. v. part I. p. 60—66.

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in great number on the vast sides of Mount Ætna, each indicating the discharge of a particular eruption. At the same time, a jet of flame and smoke issues from the main crater, proving the internal communication between it and the lava; this discharge from the summit generally continuing, in a greater or a less degree, during the intervals between eruptions. (Fig. 41. represents an ideal section of Mount Ætna; *a b* is the direct channel, and *b c* is a lateral branch.)

The pressure of 600 feet of liquid lava is required to constrain the acid of carbonate.

Let us now attend to the state of the lava within the mountain, during the course of the eruption; and let us suppose, that a fragment of limestone, torn from some stratum below, has been included in the fluid lava, and carried up with it. By the laws of hydrostatics, as each portion of this fluid sustains pressure in proportion to its perpendicular distance below the point of discharge, that pressure must increase with the depth. The specific gravity of solid and compact lava is nearly 2.8; and its weight, when in a liquid state, is probably little different. The table shews, that the carbonic acid of limestone cannot be constrained in heat by a pressure less than that of 1708 feet of sea, which corresponds nearly to 600 feet of liquid lava. As soon, then, as our calcareous mass rose to within 600 feet of the surface, its carbonic acid would quit the lime, and, assuming a gaseous form, would add to the eruptive effervescence. And this change would commonly begin in much greater depths, in consequence of the bubbles of carbonic acid, and other substances in a gaseous form, which, rising with the lava, and through it, would greatly diminish the weight of the column, and would render its pressure on any particular spot extremely variable. With all these irregularities, however, and interruptions, the pressure would in all cases, especially where the depth was considerable, far surpass what it would have been under an equal depth of water. Where the depth of the stream, below its point of delivery, amounted, then, to 1708 feet, the pressure, if the heat was not of excessive intensity, would be more than sufficient to constrain the carbonic acid, and our limestone would suffer no calcination, but would enter into fusion; and if the eruption ceased at that moment, would crystallize in cooling along with the lava, and become a nodule of calcareous spar. The mass of lava, containing this nodule, would then constitute a real whinstone, and would belong

Calcareous spar formed by volcanic fusion in the winstone.

belong to the kind called *amygdaloid*. In greater depths still, the pressure would be proportionally increased, till sulphur, and even water, might be constrained; and the carbonate of lime would continue undecomposed in the highest heats.

If, while the lava was in a liquid state, during the eruption or previous to it, a new rent (*d e*, Fig. 41.), formed in the solid country below the volcano, was met by our stream (at *d*), it is obvious that the lava would flow into the aperture with great rapidity, and fill it to the minutest extremity, there being no air to impede the progress of the liquid. In this manner, a stream of lava might be led from below to approach the bottom of the sea (*f f*), and to come in contact with a bed of loose shells (*g g*), lying on that bottom, but covered with beds of clay, interstratified, as usually occurs, with beds of sand, and other beds of shells. The first effect of heat would be to drive off the moisture of the lowest shell-bed, in a state of vapour, which, rising till it got beyond the reach of the heat, would be condensed into water, producing a slight motion of ebullition, like that of a vessel of water, when it begins to boil, and when it is said to simmer. The beds of clay and sand might thus undergo some heaving and partial derangement, but would still possess the power of stopping, or of very much impeding, the descent of water from the sea above; so that the water which had been driven from the shells at the bottom, would not return to them, or would return but slowly; and they would be exposed dry to the action of heat.* Submarine eruptions,

In this case, one of two things would inevitably happen. Either the carbonic acid of the shells would be driven off by the heat, producing an incondensable elastic fluid, which, heaving up or penetrating the superincumbent beds, would force its way to the surface of the sea, and produce a submarine eruption, as has happened at Santorini and elsewhere; or the volatility of the carbonic acid would be repressed by the weight of the superincumbent water (*k k*), and the shell-bed, being softened or fused by the action of heat, would be converted into a stratum of limestone. and formation of limestone.

* This situation of things, is similar to what happens when small-coal is moistened, in order to make it cake. The dust, drenched with water, is laid upon the fire, and remains long wet, while the heat below suffers little or no abatement.

The

The foregoing experiments enable us to decide in any particular case, which of these two events must take place, when the heat of the lava and the depth of the sea are known.

Application of the preceding experiments to determine the formation of limestone.

The table shews, that under a sea no deeper than 1708 feet, near one-third of a mile, a limestone would be formed by proper heat; and that, in a depth of little more than one mile, it would enter into entire fusion. Now, the common soundings of mariners extend to 200 fathoms, or 1200 feet. Lord Mulgrave * found bottom at 4680 feet, or nearly nine-tenths of a mile; and Captain Ellis let down a sea-gage to the depth of 5346 feet.† It thus appears, that at the bottom of a sea, which would be sounded by a line much less than double of the usual length, and less than half the depth of that sounded by Lord Mulgrave, limestone might be formed by heat; and that, at the depth reached by Captain Ellis, the entire fusion would be accomplished, if the bed of shells were touched by a lava at the extremity of its course, when its heat was lowest. Were the heat of the lava greater, a greater depth of sea would, of course, be requisite to constrain the carbonic acid effectually; and future experiments may determine what depth is required to co-operate with any given temperature. It is enough for our present purpose to have shewn, that the result is possible in any case, and to have circumscribed the necessary force of these agents within moderate limits. At the same time it must be observed, that we have been far from stretching the known facts; for when we compare the small extent of sea in which any soundings can be found, with that of the vast unfathomed ocean, it is obvious, that in assuming a depth of one mile or two, we fall very short of the medium. M. de la Place, reasoning from the phenomena of the tides, states it as highly probable that this medium is not less than eleven English miles.‡

It might be formed at less than double the usual depth of soundings.

Less depths would afford the same pres-

If a great part or the whole of the superincumbent mass consisted, not of water, but of sand or clay, then the depth re-

* *Voyage towards the North Pole*, p. 142.

† *Philosophical Transactions*, 1751, p. 212.

‡ " On peut donc regarder au moins comme très probable, que " la profondeur moyenne de la mer n'est pas au-dessous de quatre " lieues." De la Place, *Hist. de l' Acad. Roy. des Sciences*, année, 1776.

quisite

quisite to produce these effects would be lessened, in the inverse ratio of the specific gravity. If the above-mentioned occurrence took place under a mass composed of stone firmly bound together by some previous operation of nature, the power of the superincumbent mass, in opposing the escape of carbonic acid, would be very much increased by that union and by the stiffness or tenacity of the substance. We have seen numberless examples of this power in the course of these experiments, in which barrels, both of iron and porcelain, whose thickness did not exceed one-fourth of an inch, have exerted a force superior to the mere weight of a mile of sea. Without supposing that the substance of a rock could in any case act with the same advantage as that of a uniform and connected barrel; it seems obvious that a similar power must, in many cases, have been exerted to a certain degree.

sures against carbonic acid if the superior mass were heavier or firmer.

We know of many calcareous masses which, at this moment, are exposed to a pressure more than sufficient to accomplish their entire fusion. The mountain of Saleve, near Geneva, is 500 French fathoms, or nearly 3250 English feet, in height, from its base to its summit. Its mass consists of beds, lying nearly horizontal, of limestone filled with shells. Independently, then, of the tenacity of the mass, and taking into account its mere weight, the lowest bed of this mountain, must, at this moment, sustain a pressure of 3250 feet of limestone, the specific gravity of which is about 2.65. This pressure, therefore, is equal to that of 8612 feet of water, being nearly a mile and a half of sea, which is much more than adequate, as we have shewn, to accomplish the entire fusion of the carbonate, on the application of proper heat. Now, were an emanation from a volcano, to rise up under Saleve, and to penetrate upwards to its base, and stop there; the limestone to which the lava approached, would inevitably be softened, without being calcined, and, as the heat retired, would crystallize into a saline marble.

Instance in Mount Saleve, where this pressure actually exists.

Some other circumstances, relating to this subject, are very deserving of notice, and enable us still further to compare the ancient and modern operations of fire.

It appears, at first sight, that a lava having once penetrated the side of a mountain, all subsequent lavas should continue,

Ancient lavas stop up side passages thro' which other

eruptions might else have passed.

as water would infallibly do, to flow through the same aperture. But there is a material difference in the two cases. As soon as the lava has ceased to flow, and the heat has begun to abate, the crevice through which the lava had been passing, remains filled with a substance, which soon agglutinates into a mass, far harder and firmer than the mountain itself. This mass, lying in a crooked bed, and being firmly welded to the sides of the crevice, must oppose a most powerful resistance to any stream tending to pursue the same course. The injury done to the mountain by the formation of the rent, will thus be much more than repaired; and in a subsequent eruption, the lava must force its way through another part of the mountain or through some part of the adjoining country. The action of heat from below, seems in most cases to have kept a channel open through the axis of the mountain, as appears by the smoke and flame which is habitually discharged at the summit during intervals of calm. On many occasions, however, this spiracle seems to have been entirely closed by the consolidation of the lava, so as to suppress all emission. This happened to Vesuvius during the middle ages. All appearance of fire had ceased for five hundred years, and the crater was covered with a forest of ancient oaks, when the volcano opened with fresh vigour in the sixteenth century.

Even the perpendicular vent may be closed, as happened to Vesuvius for half a century.

The opening of a Volcano in many cases is effected with prodigious explosion and disruption.

The eruptive force, capable of overcoming such an obstacle, must be tremendous indeed, and seems in some cases to have blown the volcano itself almost to pieces. It is impossible to see the Mountain of Somma, which, in the form of a crescent, embraces Mount Vesuvius, without being convinced that it is a fragment of a large volcano, nearly concentric with the present inner cone, which, in some great eruption, had been destroyed all but this fragment. In our own times, an event of no small magnitude has taken place on the same spot; the inner cone of Vesuvius having undergone so great a change during the eruption in 1794, that it now bears no resemblance to what it was when I saw it in 1785.

Hence the earthquakes preceding common eruptions.

The general or partial stagnation of the internal lavas at the close of each eruption seems, then, to render it necessary, that in every new discharge, the lava should begin by making a violent laceration. And this is probably the cause of those tremendous earthquakes which precede all great eruptions, and

and which cease as soon as the lava has found a vent. It seems but reasonable to ascribe like effects to like causes, and to believe that the earthquakes which frequently desolate countries not externally volcanic, likewise indicate the protrusion from below of matter in liquid fusion, penetrating the mass of rock.

The varying resistances opposed by old lavas to the escape of new, occasions great variety in the phenomena.

The injection of a whinstone-dike into a frail mass of shale and sandstone, must have produced the same effects upon it that the lava has just been stated to produce on the loose beds of volcanic scoria. One stream of liquid whin, having flowed into such an assemblage, must have given it great additional weight and strength: so that a second stream coming like the first, would be opposed by a mass, the laceration of which would produce an earthquake, if it were overcome; or by which, if it resisted, the liquid matter would be compelled to penetrate some weaker mass, perhaps at a great distance from the first. The internal fire being thus compelled perpetually to change the scene of its action, its influence might be carried to an indefinite extent: So that the intermittance in point of time, as well as the versatility in point of place, already remarked as common to the Huttonian and Volcanic fires, are accounted for on our principles. And it thus appears, that whinstone possesses all the properties which we are led by theory to ascribe to an internal lava.

This connection is curiously illustrated by an intermediate case between the results of external and internal fire, displayed in an actual section of the ancient part of Vesuvius, which occurs in the Mountain of Somma mentioned above. I formerly described this scene in my paper on Whinstone and Lava; and I must beg leave once more to press it upon the notice of the public, as affording to future travellers a most interesting field of geological inquiry.

Remarkable facts exhibited in the vertical Cliff of Somma.

The section is seen in the bare vertical cliff, several hundred feet in height, which Somma presents to the view from the little valley, in form of a crescent, which lies between Somma and the interior cone of Vesuvius, called the *Atrio del Cavallo*. (Fig. 42. represents this scene, done from the recollection of what I saw in 1785. *a b c* is the interior cone of Vesuvius; *d f g* the mountain of Somma; and *c d e* the *Atrio del Cavello)*. By means of this cliff (*f d* in Fig. 42. and which is represented

 sepa-

separately in Fig. 44.), we see the internal structure of the mountain, composed of thick beds *(k k)* of loose scoria, which have fallen in showers; between which thin but firm streams *(m m)* of lava are interposed, which have flowed down the outward conical sides of the mountain. (Fig. 43. is an ideal section of Vesuvius and Somma, through the axis of the cones, shewing the manner in which the beds of scoria and of lava lie upon each other; the extremities of which beds are seen edgewise in the cliff at *m m* and *k k*, Fig. 42. 43. and 44.)

Explanation.

This assemblage of scoria and lava is traversed abruptly and vertically, by streams of solid lava (*n n*, Fig. 44.) reaching from top to bottom of the cliff. These last I conceive to have flowed in rents of the ancient mountain, which rents had acted as pipes through which the lavas of the lateral eruptions were conveyed to the open air. This scene presents to the view of an attentive observer, a real specimen of those internal streams which we have just been considering in speculation, and they may exhibit circumstances decisive of the opinions here advanced. For, if one of these streams had formerly been connected with a lateral eruption, discharged at more than 600 feet above the *Atrio del Cavallo*, it might possibly contain the carbonate of lime. But could we suppose that depth to extend to 1708 feet, the interference of air-bubbles, and the action of a stronger heat than was merely required for the fusion of the carbonate, might have been overcome.

Larger scale of operation at Mount Ætna.

Perhaps the height of Vesuvius has never been great enough for this purpose. But could we suppose Ætna to be cleft in two, and its structure displayed, as that of Vesuvius has just been described, there can be no doubt that internal streams of lava would be laid open, in which the pressure must have far exceeded the force required to constrain the carbonic acid of limestone; since that mountain occasionally delivers lavas from its summit, placed 10.954 feet above the level of the Mediterranean,* which washes its base. I recollect having seen, in some parts of Ætna, vast chasms and crags, formed by volcanic revolutions, in which vertical streams of lava, similar to those of Somma, were apparent. But my attention not having been turned to that object till many years afterwards, I have only now to recommend the investigation of this interesting point to future travellers.

(To be concluded in our next.)

* *Phil. Trans.* 1777, p. 595.

V.

*Instructions for building very strong and durable Walls and Houses of any dimensions of common unprepared Earth, rammed into Moulds, by the method called Pisé, which has been practised from the earlies times in the vicinity of Lyons, and elsewhere.**

IN the year 1791, a work was published at Paris, by M. Francois Cointeraux, containing an account of a method of building strong and durable houses with no other materials than earth, which has been practised for ages in the province of Lyons. It appeared to be attended with so many advantages, that several gentlemen of England, who employ their leisure in the study of rural economy, were induced to make trial of its efficacy; and the event of their experiments has rendered them anxious to extend, by all possible means, the knowledge and practice of so beneficial an art. With a view to promote this desirable end, the account contained in the following pages has been extracted from the French work; and it will be found to contain every necessary information by those into whose hands the original may not have fallen, or who, being unacquainted with the language, may have been prevented from consulting it. The appearance of those wretched hovels which are built with mud, in most parts of Ireland, will perhaps dispose many persons to doubt the strength and durability of houses whose walls are composed of no other materials than earth. The French author says, "The possibility of raising the walls of "houses two or even three stories high, with earth only, which "will sustain floors loaded with the heaviest weights, and of "building the largest factories in this manner, may astonish

Strong and durable houses built of earth only.

The method: It is totally different from that of making mud walls,

—and incomparably stronger.

* Extracted (by permission) from "Barber's Farm Buildings, containing designs for Cottages, Farm-houses, Lodges, Farm yards, &c. with appropriate scenery to each." London, printed for W. Harding, 36, St. James's Street.

It may be an acceptable piece of information for agriculturists to know, that Mr. Harding has taken considerable pains to render his shop a repository of all the works of the first character relating to the various branches of agriculture.

"every

"every one who has not been an eyewitness to such things." But it is hoped that a description of this manner of building will sufficiently explain the reason of its superiority.

The method, called Pisé, consists simply in compressing earth in moulds or cases.

Pisé is a very simple operation: it is merely by compressing earth in moulds or cases, that we may effect the building of houses of any size or height. This art, though at present confined almost to the Lyonese in France, was known and practised at a very early period of antiquity, as appears from a passage in Pliny's Natural History. M. Goiffin, who published a treatise on Pisé in 1772, is of opinion that the art was practised by the Romans, and then introduced into France: and Abbé Rozier, in his Journal de Physique, says, that he has discovered some traces of it in Catalonia; so that Spain, like France, has a single province in which this ancient manner of building has been preserved. The art, however, well deserves to be introduced into more general use: the cheapness of the materials, and the great saving of time and labour which it affords, must recommend it in all places and on all occasions. But the French author says that it will be found particularly useful in hilly countries where carriage is difficult and sometimes impracticable; and for farm buildings, which as they must be made of considerable extent, are usually very expensive.

It appears to be of considerable antiquity.

Is very economical.

Of the Implements necessary for building in Pisé.

Account of the tools requisite for the practice:

Besides the common tools, such as spades, trowels, baskets, watering-pots, plumb-rule, hatchet and hammer, the only implements required for building in pisé are a mould and a rammer, of which it will be necessary to give a particular description.

The following is a list of their several parts, as they are delineated in Plate V.

—they are a mould and a rammer.

Fig. 1. One side of the mould seen on the outside.
2. The other side seen within.
3. Head of the mould seen without.
4. The other face seen within.
5. Wedges.
6. A round stick, called the wall-gauge.
7. Posts set upright, seen flat-wise with its tenons.
8. The same on the edge.

Fig.

Fig. 9. Joists in which the mortices are cut, seen flat.
10. The same, with the side and bottom seen.
11. A mould put together, in which are seen all the parts above mentioned, and a small rope.
12. The rammer (or pisoir) for ramming the earth in the mould.
13. The same, seen on its side.

For the construction of the mould, take several planks, each ten feet long, of light wood, in order that the mould may be easy to handle. Deal is the best. Let them be ploughed and tongued, or jointed close, and planed on both sides of these planks, fastened together with four strong ledges or battens on each side: the mould must be made two feet nine inches in height; and two handles should be fixed on each side. See Fig. 1. and 2. The head of the mould, which serves to form the angle of the building, must be made of two pieces joined at the sides; its breadth eighteen inches, and height three feet. See Fig. 3. and 4., where it will be remarked that this part of the mould diminishes gradually to the top, in order that the wall may be made to diminish in the same degree. **Description of the mould,**

All the boards should be full an inch thick. The wedges must be an inch thick, and from eight to twelve inches long, and the gauge, Fig. 6. must be cut in length equal to the wall you intend to erect. **—and its several parts.**

The posts are to exceed the height of the mould by eighteen inches. They must therefore be about five feet high, including their tenons (which should be six inches long) and three by four inches thick.

The joists may be three feet six inches long, three inches and a half broad, and three inches thick. On the broad part must be made the two mortices (as marked Fig. 9.) ten inches and a half long, and full an inch wide, and at each end three inches and a half beyond the mortices; so that the interval between them will be fourteen inches.

For a further explanation, an elevation of the whole machine is annexed, Fig. 11.; and the following is a list of the several parts, in the same order that the workmen must follow when they erect the mould.

Eleva-

Elevation of the Mould on the Wall.

Method of fixing or using the mould, between which the earth is to be compressed;

A. A stone foundation, eighteen inches thick.

B. Joists laid across the foundation wall.

CC. The two sides of the mould, including between them three inches of the foundation wall.

DD. The two upright posts, the tenons of which fit into the mortices of the joists.

E. Wall-gauge which fixes the width of the mould at top, and which is shorter than the thickness of the wall at bottom, to regulate the diminution of the wall.

F. A small cord making several turns round the posts.

G. A stick, which being wound round, fastens the cord, and holds the posts tight together.

HH. Wedges which enter into the mortices in the joists, and keeps the posts and mould firmly fixed against the wall.

—and of taking it down.

Such is the process of erecting the mould. A contrary order must be observed in taking it to pieces. The rope must be loosened, the wedges taken out, and the posts, mould, and joists removed, to refix the whole again.

The rammer.

The instrument with which the earth is rammed into the mould, is a tool of the greatest consequence in the formation, on which the durability and perfection of the work depends. It is called a pisoir, or rammer. An idea of its construction may be formed by examining Fig. 12. and 13. better than by words. It should be made of hard wood, either ash, oak, beech, or walnut.

Method of Working.

The method of working, exemplified in an house constructed of earth,

Let us not confound pisé with the miserable way of building with clay or mud, mixed with straw, as practised through Ireland. Nothing can in reality be more different. Those wretched huts are built in the very worst manner that can be imagined; whereas pisé contains all the best principles of masonry, together with some rules peculiar to itself. Fig. 14. and 15. represent the elevation and plan of a house, the building of which will be regularly described, according to the method of pisé.

The

The foundation must be of masonry, eighteen inches thick, and may be raised to a foot or eighteen inches above the ground; which is necessary, to secure the walls from moisture or splash. Mark upon them the distance at which the joists are to be set for receiving the mould. These should be three feet each, from centre to centre. This will leave six inches at each end, which serve to lengthen the mould at the angles of the house. After having set the joists in their places, the masonry must be raised between them, six inches higher than the upper side of the joists. Raise the mould immediately on the masonry, as described, placing it over one of the angles of the wall. The head of it, which is to be placed against the angle, should have eighteen inches in breadth at the bottom, and only seventeen inches and a half at top. Thus the sides of the moulds will incline towards each other, and produce the necessary diminution. The wedges must then be driven in, the posts well fixed by cords, and the head of the mould secured by iron pins.

—on a low foundation of masonry.

The mould fixed.

A workman should be placed in each of the three divisions of the mould, the best workman at the angle. He is to direct the work of the other two; and by occasionally applying a plumb-rule, to take care that the mould does not swerve from its upright position. The labourers who prepare the earth must give it in small quantities to the workmen in the mould, who, after having spread it with their feet, begin to press it with the rammer. They must only receive so much at a time as will cover the bottom of the mould to the thickness of three or four inches. The first strokes of the rammer should be given close to the sides of the mould, but they must be afterwards applied to every other part of the surface. The men should then cross their strokes, so that the earth may be pressed in every direction. Those who stand next to one another in the mould, should regulate their strokes so as to beat at the same time under the cord; because that part cannot be got at without difficulty, and must be struck at obliquely. With this precaution, the whole will be equally compressed. The man at the angle of the wall should beat carefully against the head of the mould; and to encrease the strength of the building, it is usual to spread, every six inches high, a layer of mortar near the head. Care must be taken that no fresh earth is received into

Three workmen ram at the same time, in the length of twelve feet.

Method of ramming.

into the mould till the first layer is well beaten, which may be ascertained by striking it with the rammer. The stroke should leave hardly any print on the place. They must proceed in this manner, to ram in layer after layer, till the whole mould is full. When this is done, the mould may be taken to pieces, and the earth which it contained will remain firm and upright, about nine feet in length, and two feet and a half in height. The mould may then be replaced for another length, including one inch of that which has first been completed. The regular manner of joining the different lengths may be seen in the geometrical elevation, Fig. 14. Pl. VI., where it will be observed that no joints are left in this work, as the different lengths are united, and made to press one upon the other. In the second length, and most of the following, the head of the mould is useless; it is only made use of at the angles.

When the ramming is complete, the earth is quite firm.

Partition walls.

When the workmen have gone round the building, they must begin upon the partition-walls, where the head of the mould must be used to form the door-jambs.

Subsequent courses: They are so firm, that no time is lost by waiting for them to become indurated. Three courses may be raised in a day.

The first course being thus completed, we proceed to the second; and in each successive course we must proceed in a direction contrary to that of the preceding. It may easily be conceived, that with this precaution the joints of the several courses will be inclined in opposite directions, which will contribute very much to the firmness of the work. There is no reason to fear overcharging the first course with the second, though but just laid; for three courses may be laid, without danger, in one day. Mark the grooves for receiving the joists in the first course, at the distance of three feet from each other, but not immediately over the former grooves, but over the middle points between them. See Fig. 14. These grooves must be cut for the joists, and the second course completed as the former, except that it must proceed in a contrary direction, and the head of the mould and wall-gauge must be diminished, in order that the same inclination of the sides to each other that was given to the first course may be preserved in this second.

Connection of the party-wall with the side-walls.

It must, however, be remarked, that this second course is not to be continued, without interruption, like the first, as it is necessary that the partition-wall, should join or bind into the exterior wall, or rather that all walls in the building, whether outside

outside or partition-walls, which meet at an angle, should cross each other at every course. In pursuance therefore of this rule, when the work has been advanced from A to C. Fig. 15. leave the exterior wall, and turn the mould to the partition. When the work has been carried on along the partition-wall to its termination, bring back the mould to the part which remained unfinished in the exterior wall marked C; and after having filled up that space, carry on the mould beyond the partition-wall and complete the course.

This description of the two first courses is equally applicable to all the other, and will probably enable any person to build a house with no other material than earth, of any height and extent he pleases.

The gable ends.

With respect to the gables, they cannot be crossed, as they are detached from each other; but as their height is so inconsiderable, and they are, beside, connected together by the roof, this is not of any consequence.

It has been observed, that each course will be two feet and a half high, if the mould be two feet nine inches; for the mould must include three inches of the course beneath. For this reason the grooves are made six inches deep, though the joists are only three inches in thickness.

Houses built in this manner are very durable.

Such is the method of building which has been practised by the Lyonese for many centuries. Houses thus built are strong, healthy, and very cheap. They will last a great length of time: for the French author says he had pulled down some of them, which, from the title deeds in the hands of the proprietors, appeared to be above a century old. The rich traders of Lyons have no other way of building their country-houses. An outside covering of painting in fresco, which is attended with very little expence, conceals the nature of the building, and is a handsome ornament to the house. That method of painting has more freshness and brilliancy than any other, because water does not impair the colours. No size, oil, or expence is required; manual labour is almost all it costs, with a little red or yellow ochre, or other mineral colours.*

The outside coating.

Plaster and pebbles recommended for the outside.

* I would recommend the outside to be plastered and pebbled handsomely, or rough-cast, as painting in fresco is not understood in this country; and the other method would have a greater neatness. The interior to be plastered as common.

 Strangers

Strangers who have sailed upon the Rhone probably never suspected that those beautiful houses which they saw rising on the hills around them, were built of nothing but earth.

Many advantages to be derived from this manner of building.

There is every reason for introducing this method of building into all parts of Ireland: whether we consider the honour of the nation as concerned in the neatness of its villages, and the consequent health of the inhabitants, to which it will greatly contribute (as such houses are never liable to the extremes of heat or cold), or whether we regard the project on an economical or an expeditious scale, by saving both time and labour in building, and rendering the houses thus built almost immediately inhabitable after they are finished; for which latter purpose, the holes made for the joists should not be closed up directly, but left for the air to dry the walls more speedily.

Method of forming the openings for Doors and Windows.

How the door and window-ways are to be made.

The openings for the doors and windows must be left at the time of building the walls. This may be done by placing within the mould either two or one of the heads, wherever the wall is to terminate, and the opening commence. They should be made sloping a little, in order to leave room for the frames and sashes.

On the description of compressed Earth.

Experiments which shew the time of drying.

After beating a small portion of earth, it was found to weigh thirty-nine pounds and a half. Fifteen days afterwards it had lost four pounds and a quarter. In the space of another fifteen days, it lost but one pound; and in fifteen days after that, it diminished only half a pound. In the space of forty-five days the moisture was completely evaporated, and it's weight was diminished about one-eighth. This small portion of moisture cannot at all affect the solidity and consistency of the earth so treated. This experiment is also sufficient to shew the difference between this kind of building and that vulgar kind called in Ireland "Mud-walling." The latter cannot be executed without adding a great deal of water, which occupies a considerable space in the mud, and leaves, on evaporating an infinite number of pores or little cavities; and thus the walls become weak and brittle, and incapable of supporting much weight, as beaten earth or pisé can sustain.

In forty-five days the rammed earth is dry, and loses only one-eighth.

In

In a single day three courses may be laid, one over the other, so that a wall of eight or nine feet, or one story high, may be raised in one day. Experience has proved, that as soon as raised to a proper height for flooring, the heaviest beams and rafters may, without danger, be placed on the walls, and that the thickest timber of a roof may be laid on the gables the instant they are completed.

One story may be built in a day.

On Earth proper for Pisé.

First,—All earths in general arê fit for that use, when they have not the lightness of poor lands, nor the stiffness of clay.

Buildings may be made of any earth not too light or too stiff.

Secondly,—All earths fit for vegetation.

Thirdly,—Brick earths.

Fourthly,—Strong earths, with a mixture of small gravel, which for that reason cannot serve for making either bricks, tiles, or pottery, but make the best pisé.

The following appearances indicate that the earth in which they are found is fit for building. When a pick-axe, spade, or plough brings up large lumps of earth at a time; when arable lands lie in clods or lumps; when field-mice have made themselves subterraneous passages in the earth; all these are favorable signs. When the roads of a village, having been worn away by the water continually running over them, are lower than the other lands, and the sides of those roads support themselves almost upright, it is a sure mark that the pisé may be executed in that village.

How to chuse the kind of earth,

Proper earth is found at the bottom of the slopes of low lands that are cultivated, because every year the rain brings down the good earth. It is also found on the banks of rivers. If the earth to be had is not quite fit, it may be mixed to make it so. Strong earths must be tempered with light; those in which clay predominates, with others composed more of chalk and sand; and those of a rich substance with others of a poor nature. It will not be amiss to mix with the earth some small pebbles, gravel, rubbish of mortar, or any small mineral substances; but none of the animal or vegetable kind must be admitted. Such hard substances bind the earth firmly between them; so that a well worked earth, in which there is a mixture of gravel, becomes so hard at the end of two years that a chisel must be used to break it, as if it were freestone.

—and to improve it if required.

The old walls are as hard as a stone.

Experi-

Experiments to ascertain the qualities of any Earth.

Simple experiments by which the goodness of any earth may be tried.

Take a small wooden tub without a bottom, dig a hole in the ground, fix a piece of stone or flag at the bottom, place the tub upon the stone, fill round it the earth you dug out and ram it well to prevent the tub from bursting: then ram into the tub the earth you intend to try, a little at a time, as described. When the tub is full, loosen the earth around it, and take it out with the compressed earth in it, then turn the tub upside down, and the pisé will come out. If not immediately, let it stand to dry, and it will fall out of itself. Leave the lump to stand some time; and if it do not crack, but increase in hardness, it is fit for building. This experiment may be made in a small box, in the hand. Every person in walking over his grounds may make little balls of earth, and press them, as firmly as he can, between his hands. If he brings them home, and puts marks on them, he will by that means know the quality of every piece of land, and also be a judge of the mixture it will be necessary to make.

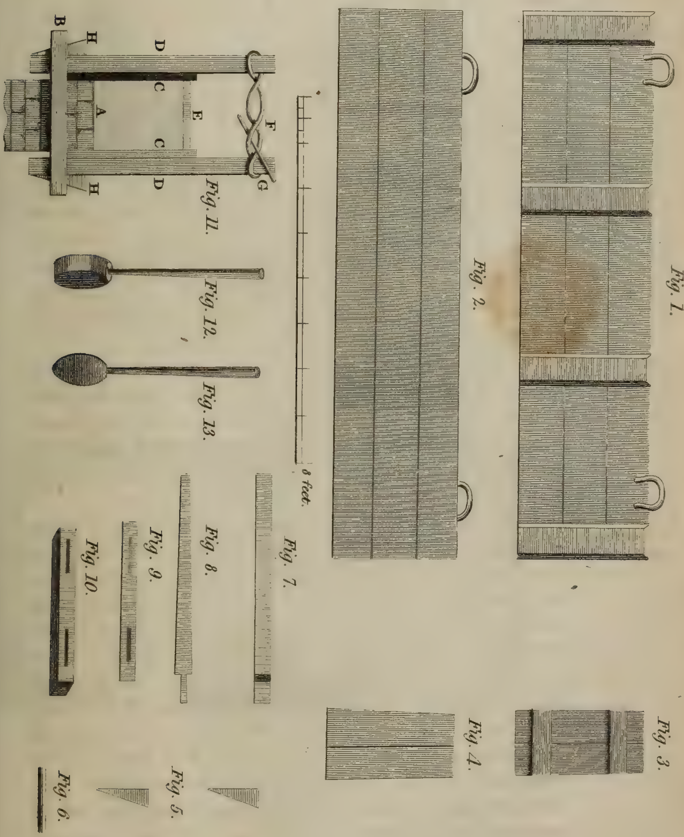
Simplicity of this art.

All the operations of this art are simple and easy. There is nothing to be done but to dig up the earth, break the clods with a shovel, and to lay it in a heap, where the large lumps are to be drawn away by a rake, in which there may be intervals of an inch and a quarter between the teeth, that the stones and pebbles, of the size of a walnut, may remain.

Binders or bond-pieces of wood.

It is necessary to lay in binders or bonds when the first course is laid and the mould fixed for the succeeding one. Lay in at the bottom of it a board, rough from the saw, about five or six feet long, eight or nine inches broad, and about an inch thick. There will be some inches of earth on each side, by the wall being so much thicker. This will entirely conceal the board in the wall and prevent its rotting. In the next course, or in the middle of the mould, there may be short ends of boards, laid across so that they shall not come through, but be concealed also in the wall. These may be at two or three feet intervals, and crossing each other at the angles. This will serve much to equalize the pressure. When the wall is completed to the height of a story, boards of three or four feet in length should be placed on the pisé, in those places where the

Cheap and durable Buildings of Earth.



Cheap and durable Buildings of Earth.

Fig. 14.

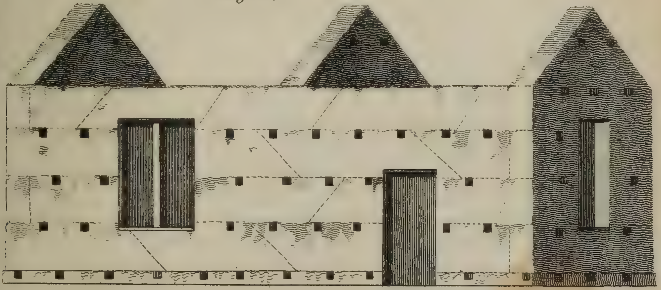
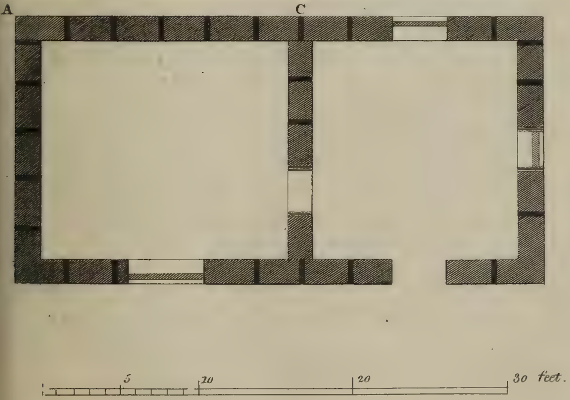


Fig. 15.



the beams and rafters are to be fixed, which may be laid on as soon as the mould no longer occupies that place.

With respect to walls for very large enclosures, there may be two moulds and two sets of workmen to expedite.

Facts which shew how expeditiously this kind of building is performed.

Beside the advantage of strength and cheapness, this method of building possesses that of speed. To give an idea of the time that is necessary to build an house or an enclosure, a mason used to the work, can, with the help of his labourer, build in one day, four square yards, or thirty-six square feet of pisé; therefore six men, which is the necessary number to work the mould, will build in one day, twelve square yards,* or one hundred and eight square feet; or in one week, six hundred and forty-eight feet. By this it is easy to calculate the time necessary for building a house or wall of any dimensions. These facts, which have been proved by numberless instances, afford a criterion by which every one may determine the time that his house or wall will take in building, having first ascertained the number of feet it will contain. Thus if he wishes to have a wall built five hundred and forty feet long, and six feet high, with one mould and six men, it will finish eighteen feet running measure, or one hundred and thirty-eight square feet in a day. It will be completed in thirty days, comprehending in the whole, three thousand two hundred and forty square feet.† As to the expence, it being only the labour, except the mould and the materials (scarcely any thing), the five common labourers at 1s. per day, and the conducting builder at 2s. makes 7s. per day. It will be finished for £10. 10s.

Six men will build in a day a wall eighteen feet long and six feet high.

Three thousand two hundred and forty square feet for about ten guineas.

Plastering, &c.

The plastering and rough-casting or dashing, should not be done for five or six months after the walls are built; and they should always be built between the months of March and October inclusive. To prepare the walls for plastering, indent them closely with the point of a hammer or hatchet.

* This is the calculation as translated from M. Cointeraux; which to render more familiar to building in this country, is three and a half perches.

† One hundred and four perches.

VI.

VI.

Considerations on the Oxidation of Metals in general, and on that of Iron in particular. By M. Thenard.*

Degrees of oxidation in metals constant, determinable by the salts they form, and numerous.

THE object of the author in this memoir is to show, that the degrees of oxidation in metals are constant, and determinable particularly by the nature and properties of the salts they form. These different degrees of oxidation are frequently pretty numerous, as in antimony, iron, and manganese.

Colours of metallic salts do not indicate those of the oxides they contain.

Iron has no yellow oxide,

but green, red, and white.

He adverts to the principle but lately known to chemists, that the colours of salts do not always indicate those of the metallic oxides they include; and this principle he applies to the study of the different oxides and sulphates of iron. Though he does not admit the existence of the yellow oxide of iron, which has been adopted from the observation of some yellow salts of this metal; he distinguishes in it three degrees of oxidation, namely, the green oxide, the red oxide, and a third made known by M. Thénard himself, which is white, and less oxided than either of the other. This is the first obtained, when we decompose a fresh solution of sulphate of iron by means of an alkali. On this occasion we see a white oxide thrown down, which quickly turns green, and even red, by absorbing oxigen.

Each oxide produces two sulphates, making six in all.

This white oxide is capable of producing two different sulphates by combining with two different proportions of sulphuric acid: and as these two degrees of saturation are equally capable of taking place with each of the other oxides, we have six sulphates of iron, observes M. Thenard, very distinct from each other, and of importance to be known, on account of the various and delicate uses to which this salt is applied in the arts. The following are the names and characters of these six sulphates:

Acidulous sulphate of white iron.

1. Acidulous sulphate of white iron. This is the white oxide just mentioned, combined with a little sulphuric acid in excess. Its colour is a deep bottle-green. It is the sort most common in the shops.

* *Bulletin des Sciences,* August, 1805, p. 223.

2. Acid

2. Acid sulphate of white iron. This is of an emerald green. It contains a much greater excess of acid, and is rejected in almost all the arts in which sulphate of iron is employed. The acidulous may be converted into the acid sulphate by the addition of a little sulphuric acid; and the acid sulphate may be converted into the acidulous by heating it with iron filings. Acid sulphate of white iron.

Alkalis precipitate both these sulphates white: substances that easily part with their oxigen, as oxigenated muriatic acid, air, water, &c. decompose them, and precipitate a green or red oxide. Common characters.

3. Acidulous sulphate of green iron. This is made by combining sulphuric acid with green oxide of iron. This salt does not crystallize; and is red, notwithstanding the green colour of its oxide. Acidulous sulphate of green iron.

4. Acid sulphate of green iron. This is nearly colourless. It is obtained by the addition of a little sulphuric acid to the preceding sulphate; and crystallizes, though with difficulty. The crystals approach the emerald green of the acid sulphate of white iron; are neither efflorescent nor deliquescent; and absorb the oxigen of the atmosphere but slowly. Acid sulphate of green iron.

Both these sulphates are precipitated green by alkalis: the iron they contain is converted into the state of white oxide by the addition of iron filings, and to that of red oxide by oxigenated muriatic acid. Common characters.

5. Acidulous sulphate of red iron. M. Thenard calls this also neutral sulphate of highly oxided iron. It is yellow, completely insoluble, and consequently incapable of being crystallized. It precipitates in the form of a yellow powder from solutions of the acidulous sulphates of white or green iron. This salt has been taken for a yellow oxide of iron, different from the red and the green. Acidulous sulphate of red iron, or neutral sulphate of highly oxided iron. Mistaken for a yellow oxide.

6. Acid sulphate of red iron. This is obtained by dissolving red oxide of iron in sulphuric acid diluted with water. It contains a greater excess of acid than the other two acid sulphates; is almost colourless, but assumes a pretty deep red if its excess of acid be saturated by potash; and does not crystallize. Acid sulphate of red iron.

Such are the principal properties of the six sulphates of iron distinguished by M. Thenard. Most of the other acids act nearly in the same manner on iron, and the three degrees of oxida- Most of the other acids act nearly in the same manner on iron.

oxidation of iron mentioned above are equally observable in the gallates and prussiates of this metal.

Gallates of iron.

The gallate of white iron, which may be obtained by decomposing the deep green sulphate of iron, is itself colourless: the gallate of green iron is blue: the gallate of red iron is black. They may also be obtained by decomposing the acidulous or acid sulphate of green or red iron by means of the gallic acid.

Prusisates still more numerous, owing to the presence of prussiate of potash.

The combinations of iron with the prussic acid exhibit much more numerous varieties, which depend not only on the different oxides of iron already mentioned, but on the greater or smaller quantity of acid, and on the presence of the prussiate of potash that may remain combined with the prussiate of iron.

Prussiate of white iron.

The prussiate of white iron is that in which the iron is in the state of white oxide, and the oxide is in excess, owing to the excess of alkali contained in the prussiate of potash.

Green prussiate of iron.

The prussiate of green iron is the same prussiate as the preceding without excess of oxide.* They both contain too, as M. Berthollet has shown, prussiate of potash, which is strongly adherent to them.

Prussiates of green and red iron.

From the ferruginous salts with bases of green and red oxides of iron, two prussiates each are equally obtainable, one with, and the other without excess of oxide. The prussiates obtained with the green oxide are blueish; those with the red oxide are of a fine blue. The six prussiates above mentioned, are capable of exhibiting farther varieties in consequence of the prussiate of potash they may contain.

Improvements in manufacturing prussian-blue.

M. Thenard concludes his memoir with proposing some means of improving the fabrication of prussiate of iron, or prussian-blue. These consist, 1st, in turning to account the great quantity of ammonia formed by the calcination: 2d, in employing the most advantageous proportion of potash, which appears to consist in equal parts of blood and alkali: 3d, in adding iron during the evaporation, which facilitates the formation of the prussiate of potash: 4th, in causing the prussiate of potash to crystallize.

* Either M. Thenard here departs from the principles of his nomenclature, and means to call this the *green prussiate of iron;* or the abridger of his memoir has fallen into some mistake. T.

VII.

VII.

Abstract of a Memoir on the Dyes obtained from different Species of Clubmoss, Lycopodium. *Translated from the Swedish of* Dr. J. F. Westring, *of Nordkoeping, Physician to the King of Sweden, by* Eugene Coquebert.*

THE numerous experiments Dr. Westring has made on the colouring properties of lichens, and the interesting discoveries that have resulted from his inquiries, are well known. Attempting to fix one of these dyes of a very fugitive nature, he bethought himself of employing as a mordant the species of moss known to botanists by the name of *lycopodium complanatum*;† he did not attain the object he had in view, but a result which he did not expect: for he discovered, that a very fine blue, considerably permanent, might be struck on wool or silk, by boiling it first with this species of lycopodium, and then steeping it in a slight infusion of brasil.

Lycopodium complanatum, gives a fine blue to wool and silk, with the addition of a little brasil.

The wool treated in this manner was at least of as fine a blue as if it had been dyed with woad, or in what dyers commonly call the vat; and was so fixed, that on rubbing a piece of white linen with it, it did not stain it as many blue cloths do; that being rinsed in cold water, it did not impart to it the slightest blue tinge, and that it resisted the action of boiling soapsuds.

Equal to woad, and fixed.

The only inconvenience of this dye is its being injured by all acids, even common vinegar, which redden it more or less: but it is easy to remove the spots thus produced by means of a weak alkali, which will restore the colour, and occasion no change itself.

Acids spot it, but the spots removed by alkali.

The following manner of employing the lycopodium complanatum Dr. Westring has found to be the most simple and convenient:

Take a quantity of this moss, dried and chopped, nearly double the weight of the cloth to be died. Put them into a proper vessel, a stratum of the moss between every fold of the cloth,

Method of using it.

* *Bulletin des Sciences,* August, 1805, p. 224

† The country people in Sweden sometimes use a species of lycopodium as a mordant. T.

cloth, and pour on a sufficient quantity of water, which must be at least sufficient to cover the whole well. Boil them together for two or three hours, adding more water from time to time, to supply the place of what is wasted by evaporation. Take out the cloth thus prepared, wring it, and hang it up to dry without rinsing.

Afterward, when you would dye the cloth thus prepared, begin by rinsing it carefully in cold water; then put it into a well tinned copper, with cold river or spring water, and a small quantity of brasil; and boil it gently for half an hour or an hour, according as you would have the tint deeper or lighter. If too much brasil be used, the dye will have a violet hue.

After having taken it from the fire, rinse the cloth immediately in cold water. It is not necessary that the bath should have been made to boil; it is sufficient to keep it for a couple of hours at a heat of 60° or 70° of the centesimal thermometer (140° or 158° Farht).

None of the common mordants must be used.

The brasil may be mixed at once with a strong decoction of lycopodium: but in either case, care must be taken that none of the common mordants, either saline or astringent, are used, for they would alter the colour.

Recommended for the army clothing.

Dr. Westring conceives, that this process may be substituted for the common mode of dyeing the cloth used for troops, with a saving of expence.

The lycopodium is very common in the Swedish woods, so that it would furnish an article of exportation, beside an abundant supply for home consumption.

Lycopodium clavatum, answers the same purpose.

Dr. Westring has extended his experiments to the various species of this genus. He has found that the *lycopodium clavatum*, which is more common than the *complanatum*, may be used in the same manner with equal advantage. The blue it yields, when it is perfectly dry, is even deeper, which may render it preferable. Hitherto this moss has been applied to no use but that of making mats, except that the farina of the stamens has been sometimes employed medicinally.

Lycopodium annotinum yields grays. A good mordant.

The *lycopodium annotinum* does not yield a blue with brasil-wood, but several shades of gray, in which acids and alkalis have the effect mentioned above. This species affords a mean of easily fixing in cloth several colours, that have hitherto been found difficult to render permanent.

The

The *lycopodium selaginoides* is less common, and, like the *lycopodium selago*, yields no blue, but a fine gray, the shades of which may be varied, and which inclines to blue or violet. Lycopodium selaginoides and selago give grays.

Analogy led to the supposition, that the *lycopodium alpinum*, which covers the high mountains of Lapland like a carpet, and greatly resembles the *complanatum*, might likewise be used for dyeing blue. This Dr. Westring found to be the fact; and it appears, that the colour it affords is less injured by acids. Lycopodium alpinum gives a blue, less injured by acids.

Thus all the species of this genus will be of use in dyeing. Dr. Westring presumes, that they may be employed not only with brasil-wood, but with several other dyes, as substitutes for galls, and the salts used as mordants. He imagines too, that the barks of some of the trees indigenous to Sweden, might be found to answer as well as brasil with the lycopodium. The fresh bark of the branches of the ash yields with the *lycopodium complanatum* a changeable colour, which inclines to brown and blue, as Dr. Lindenstolpe announced as early as 1720, in a treatise on dyeing; but when this bark is green, it gives only a fine yellow, of no use as a dye. All the species useful as dyes, or as mordants. Ash bark with lycopodium complanatum.

M. Lasteyrie has received from Dr. Westring a pattern of wool dyed blue by means of the *lycopodium complanatum;* and this pattern has been shown to the Philomathic Society. Among these, the Dr. sent with his original memoir to the Patriotic Society of Sweden, there was some silk, which, treated in the manner above described, had taken a fine blue colour inclining to red, which the dyers have called *œil de roi.* If the quantity of brasil used be greater, the silk acquires a *puce* colour. Patterns. Silk, œil de roi, and puce.

Subjoined are some more facts taken from Dr. Westring's memoir, and his letter to M. Lasteyrie.

The *lichen parellus* is the only one of the lichens in which Dr. W. has perceived the property of affording a blue dye. To obtain it, all that is necessary, is to infuse this lichen in river water, without any mixture, at a temperature of 40° or 50° (104° or 112° Farht.). In three days, half an ounce of this lichen will have imparted a sufficient colour to a quart of water, and is capable of colouring three or four quarts in succession. But Dr. Westring could not fix this dye by means of any of the known mordants, or of the *lycopodium.* It even disappears as soon as the water is made to boil. Lichen parellus gives a blue, but not permanent.

The

Plumtree bark a fine carmelite.

The bark of the fresh branches of the plumbtree, taken off after the first frosts, has yielded a good dye of a fine carmelite colour. That of the Italian poplar, *populus dilutata*, whether fresh or dry, gives a permanent yellow both to wool and silk, and deserves to be employed in the great.

Populus dilatata a yellow.

This owing to the lycopodium,

This advantage is certainly owing to the preparation with lycopodium; for M. Dambourzey could obtain only a false colour of no permanence from the same bark used fresh. Yet he employed as a mordant the nitro-muriatic solution of tin, or composition, as it is called by the dyers; an addition which Dr. Westring has found to heighten the beauty of the dye.

and composition enhances the beauty.

Lycopodium with different lichens.

Wool prepared with lycopodium receives from the *lichen Westringii* a good dye of a fine orange colour, much superior to that given by annotta. The same colour is obtainable with the *lichen cinereus;* and a fine bright yellow with the *lichen chlorinus. Achart.* If the wool thus dyed be afterwards dyed with brasil, that which was prepared with the *lichen Westringii* becomes a very deep blue-black; and that with the *lichen chlorinus* of a fine green-black, or raven's-wing. With the *lichen vulpinus* the colour is of a fine lemon-yellow, which is changed by the addition of brasil to a blueish-green.

Cotton dyed with mahogany.

Dr. Westring says in his letter to M. Lasteyrie, that he has prepared with the *suretenia mahagoni*, mahogany, an aurora dye for cotton. He adds, that, having made some experiments with lichens sent by M. Lasteyrie from Auvergne, he found, that the *lichen parellus* contained no colouring matter, and that the red colour commonly ascribed to it was afforded by other lichens.

Lichen parellus no colour.

Pine bark an excellent tonic.

Dr. Westring has found, that the bark of the pine tree (Scotch fir?) is an excellent tonic: that it may be used with advantage in several convulsive diseases, even epilepsy; and that it may be substituted for the cinchona.

—bread made from it,

This bark, as is well known, is nutritious, and the inhabitants of the northern provinces of Sweden are sometimes obliged to make bread of it.

and from bog-moss, which contains much sugar.

Bread too has been made in Iceland with the *sphagnum palustre*, which is white, and said to be little inferior in taste to common bread. A surgeon of Ulesborg has found a considerable quantity of saccharine matter in this species of moss.

VIII.

VIII.

On the supposed Discoveries of LAVOISIER. *In a Letter from* E. D.

To Mr. NICHOLSON.

SIR,

Edin. June 13th, 1806.

IN addition to my remarks on M. Lavoisier's claims to the discovery of *oxidation*, inserted in your last number, I now send you some other facts on that subject, and on acidification, together with such observations, as will, I hope, tend to ascertain pretty correctly the justice and true extent of his pretensions.

Additional remarks on M. Lavoisier's pretensions to chemical discoveries.

M. Lavoisier himself tells us, that in the year 1630, J. Rey, a physician at Bugue, in Perigord, combated the opinion of Cardan and others, concerning the cause of the augmentation of weight of metallic oxides: he shewed that it did not proceed from the condensation of the soot in the furnace, nor from the vessel, nor from any emantion of the charcoal, nor from the humidity of the air: but by *conclusive reasoning* he maintained, that the increase of weight arose from the air of the vessel, which attaches itself to the minutest molecules of the calx, in the same manner as water does to sand, adhering to the smallest grains, and rendering them heavy. These opinions of Rey were afterwards quite forgotten: and Boyle and Lemery attributed the augmentation of weight to the fixation of fire.*

The doctrine of John Rey, that metals acquire weight in oxidation from the air.

Mayow, in his tract de Sal-Nitro et Sp. Nit. Aereo, published in 1674, has these words, in speaking of the calcination of antimony: "Neque illud prætereundum est, quod antimo-"nium, radiis solaribus calcinatum, haud parum in pondere "augetur, uti experientiâ compertum est: quippe vix concipi "potest, unde augmentum illud antimonii, nisi a particulis "nitro-aereis, igneisque ei inter calcinandum infixis, procedat. "Plane ut antimonii fixatio, non tam a sulphuris ejus externi "absumptione, quam a particulis nitro-aereis, quibus flamma "nitri abundat, ei infixis provenire videatur."†

Mayow ascribed this augmentation to nitro-aereal particles from the air.

In

* Phil. Journ. Jan. 1806, p. 82.

† Tractat. quinque, p. 28--9. (In English) Neither must it be overlooked, that antimony, calcined by the solar rays, is not a little increased

Dr. Hales not only proved the absorbed air to be the cause of increased weight of calcined metals; but his doctrine was noticed by others.

In my former communication, I stated that Dr. Hales, from the most decisive experiments, shewed that the increase of weight in calcination arose from the *attraction* and *condensation* of air: and that his conclusion obtained the notice of philosophers, is sufficiently evident from the following passage taken from the treatise of the very learned and ingenious Bishop Berkeley: "Fire," he says, "collected "in the focus of a glass operates in vacuo, and therefore is "thought not to need air to support it. Calx of lead hath "gone off with an explosion in vacuo, which Niewentyt and "others take for a proof that fire can burn without air. But "Mr. Hales attributes this effect to air enclosed in the red-"lead, and perhaps too in the receiver, which cannot be per-"fectly exhausted. When common lead is put into the fire, "in order to make red-lead, a greater weight of this comes out "than was put in of common lead: therefore the red-lead "should seem impregnated with fire. Mr. Hales thinks it is "with air." *

Bayen's inference that metals are oxided by air.

In the same year, viz, 1774, in which M. Lavoisier published his experiments on oxidation, M. Bayen delivered the following opinion: "Les expériences que j'ai faites me force de "conclure que dans la chaux mercuriale dont je parle, le "mercure doit son *état calcaire*, non à la perte du phlogistique "qu'il n' a pas essuyée, *mais à sa combination intime avec le "fluide élastique*, dont le poids ajouté a celui du mercure est la "seconde cause de l'augmentation de pesanteur qu'on observe "dans les précipités que j'ai soumis à l'examen." It was in consequence of hearing Bayen's paper read, says Dr. Thomson, that Lavoisier was induced to turn his attention to the subject."†

It

increased in weight, as is found by experiment: for it is scarcely possible to be conceived, whence that increase of the antimony can proceed, unless from the nitro-aereal and fiery particles fixed in it during calcination. It is plainly seen, that the fixation of the antimony arises not so much from the separation of its sulphur, as from the nitro-aereal particles with which the flame of nitre abounds, and which become fixed in it (viz. the antimony).

* Siris, 2d. edit. an. 1774, p. 91.

† Jour. de Phys. 1774. p. 295. The first memoir of Bayen, published in the Journal de Physique, for Feb. 1774, is intitled *Sur le Phlogis-*

It is manifest, therefore, from the foregoing detail of facts, that Rey first rightly attributed the weight acquired in the oxidation of metals to the attraction of air: that Mayow next sup-

The discovery was first made by Rey, and afterwards by Mayow, Hales, and Bayen.

Phlogistique, and contains a very elaborate examination of the facts relating to sulphur and the metals, upon which that doctrine was established. His second, *On some Mercurial Precipitates*, was published in April following. The precipitate alluded to in the text, was prepared with nitrous acid. In his third memoir, published in the same work, for Feb. 1775, among other experiments, he states the reduction of precipitate per se (or mercury oxided in the air), and says that the weight of the air he obtained from it was very nearly the same as that lost by the reduction. He, therefore, *doing justice by refering to John Rey*, ascribes the calcination to that air having been absorbed. In the month preceding (viz. Jan. 1775) M. Bayen, with high commendation, gives an abstract, with the full table of contents, of the work of John Rey, who was directed to the fact of the increased weight of the oxide of tin by *Brun*, apothecary at Bergerac.

M. Lavoisier's memoir, "On the nature of the principle which "combines with metals during their calcination, and which aug-"ments their weight," was read at the Royal Academy on the 26th April, 1775, and was published in Rozier's Journal for May, in the same year. In a note, the author says that the first experiments relative to that memoir were made above a year earlier, and that those upon mercury precipitated per se were first tried by the burning-glass, in November 1774, and afterwards made with all the requisite precautions at the end of Feb. 1775. In the experiment here related, the precipitate was reduced, by fire, in a closed glass vessel; and the properties of the air disengaged, are detailed with perspicuity and conciseness. He considers it as more fit for combustion, and more respirable than common air, of which he takes it to be a part.

It seems proper to remark in this place, that Dr. Priestley expelled air from precipitate per se by solar heat, Aug. 1, 1774, and was extremely surprized at the vigorous combustion it produced: that in October following, being at Paris, he often mentioned his surprize at this kind of air to M. Lavoisier, M. Le Roy, and other philosophers in that city: and that this month immediately preceded the very November in which M. Lavoisier informs us that the same experiments were *d'abord tentées au verre ardent* by himself, without making the least mention of Priestley. See Priestley on Air, in 3 volumes, vol. ii. p. 109, and Rozier's Journal before cited, v. 429.—W. N.

ported the same opinion, and added, that it arose from a pecu-liar part (the nitro-aerial) of that air: that Hales by experiment shewed the same condensation of air to take place, which Bayen* also by experiment afterwards confirmed. Lastly, it was proved by Hales, that the air, previously condensed in the process of oxidation, was liberated when the oxided metal was again submitted to heat: and Lavoisier afterwards observed, that "on operating the reduction of litharge in close vessels, "with *Hales' apparatus,* there was at the moment of the pass-"age of the calx into the metallic state a disengagement of "air in considerable quantity, at least 1000 times greater in "volume than that of the air employed."† No one, however, but Mayow as yet supposed that the oxidation was effected by a particular part of the air only; nor was it till after the brilliant discovery of oxigen gas by Dr. Priestley, that M. Lavoisier, in repeating his experiments, found that it truly depended on the combination of that gas, and the weight acquired by the metal corresponded to that which the air had lost. Hence then the steps leading to the *theory of oxidation* have, like those in most other physical discoveries, been slow and successive: and M. Lavoisier did not advance one step beyond his predecessors, until he was made acquainted with what they were ignorant of,—viz. the true composition of atmospheric air.

—and it was not till after the discovery of oxigen by Priestley, that Lavoisier followed Mayow in ascribing oxidation to it.

Facts observed and experiments made, are of equal authority. John Rey commanded both, and drew sagacious conclusions from them.

That M. Lavoisier should deny his countryman Rey the merit of discovery because he attained to his conclusion by the force of reasoning, independent of experiment, is not a little singular; for as you, Sir, justly remark, "between the obser-"vation of well established facts, and the making of direct ex-"periments, there seems to be no essential difference."‡ If, indeed, experiment alone could entitle any one to the claim of discovery, and it were the only mode by which truth could be established, what would become of the sciences of the natural historian and astronomer, for they *observe* only, but do not *experiment;* and yet the science of astronomy has attained to greater certainty and perfection than any other can boast. Experiment does not supply the place, much less supersede

* System of Chemistry. vol. i. p. 83, 1st edit.

† Phil. Jour. loc. cit.

‡ Tractat. quinq. p. 2—4.

obser-

observation; it only aids its deficiencies, corrects its errors, or hastens its results: and it is surely of but little importance, whether the experiment be conducted by the spontaneous movements of nature, or by putting her to the torture, as Bacon says, by the efforts of human ingenuity, provided the observation of its phenomena be equally exact and the conclusion be with equal accuracy obtained. But in truth, Rey did make experiments; for what was the burning of a metal in a vessel but making an experiment; and what was the conclusion he drew, contrary to the opinion of preceding authors, but the result of a sagacious observation of the phenomena which that experiment exhibited? He went half way in the discovery of oxidation; and Dr. Hales completed it, by shewing that the condensed air was again liberated by heat. M. Lavoisier's claims go no farther than having ascertained the facts with greater accuracy, and rendered them of more extensive application.

The theory of acidification is stated with considerable perspicuity by Mayow and the earlier chemists.

With regard to the *theory of acidification*, the claims of M. Lavoisier are not much better founded; for the following circumstances will shew that others divide with him that honour. Mayow long ago proved that nitre consisted of an alkaline salt derived from the earth, and of an acid spirit, and that the contact of air with the soil was essential to its production.* At first he considered the acid spirit to be derived wholly from the air, but afterwards from its great density, held that only its more active part was obtained from that source.† Boyle having proved that something essential to combustion existed in the air, Mayow called this something the igneous particles of the air, and contended that these same particles existed in nitre; for that in *vacuo* sulphur would not burn, but when mixed with nitre, it would burn either in vacuo or under water.‡ He afterwards concludes that the aerial part of nitre is nothing else but these igneo-aereal particles, and that they reside not in the alkaline base, but in the acid spirit of the nitre, to which the caustic nature of that spirit is owing.§

* Tractat. quinq. p. 2—4.
† Ibid. p. 11.
‡ Ibid. p. 13.
§ Ibid. p. 19.

Dr.

Other facts concerning acidification.

Dr. Hales remarked that acid sulphureous fuel attracts and *condenses* air, from which it may be inferred that he had actually detected an acid in the combustion of sulphur; but the acid principle he seems to have considered as residing in the sulphur and not in the air. I have not by me the late Professor Robison's edition of Dr. Black's Lectures, but I think I remember to have there seen, that Dr. Rutherford, Professor of Botany in this University, was led, from experiment, to form the same conclusion concerning the formation of sulphuric acid, as M. Lavoisier afterwards maintained; and that nothing but the rooted prejudice of Mr. Robison and others prevented Dr. Rutherford from anticipating M. Lavoisier in this theory of acidification.

Lavoisier, with great advantages of previous discovery by others, has shewn much ability in general arrangement and induction, but little disposition to display those prior claims.

Next in order came M. Lavoisier, who having repeated the experiments of Boyle and Hales on the calcination of metals, and ascertained that oxigen in all cases combined with them, extended his views to the combustion of certain inflammable substances, and found the product, instead of calx, to be a fluid or gaseous substance, possessed of acid properties. Undoubtedly the confirmation and extension of these facts are highly creditable to the industry and sagacity of Lavoisier: but from what has been already stated, it appears that Mayow first shewed that, to the constitution of the acid spirit of nitre, a certain portion of the air of the atmosphere was necessary; that Hales had in all probability found this acid by the combustion of sulphur; and that Rutherford had not only done the same thing, but had drawn the proper conclusion from it. Be it remembered also, that the great fact essential to complete the explanation of acidification, viz. the discovery of oxigen gas, was known to Lavoisier, but at the time unknown to all these authors; and that, in truth, oxidation and acidification, effected by combustion, are only particular examples of that more general law which M. Lavoisier so successfully laboured to establish,—viz. that in every case of combustion, oxigen combines with the burning body, and forms various compounds according to the nature and composition of that body. The nature, however, of these compounds, the phenomena which attend their formation, and all the variation of circumstances with which, and under which, they take place, have not hitherto been distinctly traced out and ascertained; and until this be

be done, we must, I fear, look in vain for any thing like a *complete theory of combustion.*

The theory of combustion, as well as of acidification, yet imperfect.

But whilst some substances, as sulphurated hydrogen, are found to possess acid properties, though they contain no oxigen; and many inflammable and metallic bodies unite with oxigen during combustion, and yet have nothing of the character of an acid; we must either deny that oxigen is universally the acidifying principle, or that the ordinary characters by which acids are distinguished are arbitrary and false. "If we lay it "down as an axiom that oxigen is the acidifying principle, we "must either include among acids a great number of bodies "which have not the smallest resemblance to those substances "which are at present reckoned acids, or exclude from the "class several bodies, which have the properties of acids in "perfection. The class of acids being perfectly arbitrary, "there cannot be such a thing as an acidifying principle in the "most extensive sense of the word."* But if the acidifying principle be unknown, it must be held premature in M. Lavoisier to claim the discovery of the *theory of acidification;* and his merits, therefore, in this case will consist not in forming a just and general theory, but in improving and extending our knowledge of the facts from which, perhaps, a true theory may hereafter be deduced.

The new nomenclature of chemistry has caused various chemical theories to be admitted without sufficient examination.

I have sometimes thought, that it might be well to indulge M. Lavoisier and his associates in their present unqualified claims to the theories of modern chemistry, and of physiology founded on chemical principles, from a conviction that the duration of these theories will be temporary only, and that many parts of them ere long will undergo a complete revolution; and that, therefore, with the fall of the theories, the claims would necessarily cease. It is the introduction of the new nomenclature that has chiefly supported these claims; and their authors seem to have imagined that by giving a *new name* they established a right to the *thing* which it was meant to designate. But as if to expose and punish their injustice and presumption, later discoveries are daily proving their theories are false and insufficient, and reducing their new names to mere arbitrary terms, little better, in some respects, than those which they supplanted. Oxigen is not proved to be the acidifying

* Thomson, ii. 5, edit. 1.

prin-

principle, as its etymology would imply: azotic is not the only gas which takes away life; and purified charcoal does not form carbon, as Lavoisier supposed. But the *facts* on which these theories have been raised will remain, when the theories shall have passed away; and of these, the discoveries of Scheele and Priestley exceed in ingenuity, diversity, and number, those of M. Lavoisier and all his associates; and for originality, precision, and importance, what have they to produce at all comparable to the discoveries of Cavendish and Black. Whatever may be the fate of other republics, it is devoutly to be wished that that of letters and of science may for ever stand; and when its rights are thus openly invaded, it cannot, surely, be thought unbecoming in us, the countrymen of Bacon, Boyle, and Newton, to stand forward in its defence, lest the shades of those immortal names rise up in judgment against us!

I am, Sir,

Your obedient servant,

E. D.

XI.

Facts towards forming a History of Gold. By Professor PROUST*.

Oxigen requisite to the solubility of gold.

TO ascertain the quantity of oxigen which gold requires for its solution in acids, is a point essential to be determined in the history of this metal, and I found it attended with more difficulty than I had expected.

800 p. muriatic acid, and 200 nitric, dissolve 187 of gold.

Six hundred grains of muratic acid at 12° of Baumé's hydrometer, and 200 of nitric acid at 40°, dissolved by the assistance of heat 144 grains of gold. Having added 200 grains of muriatic acid to the solution, it took up 43 grains more of gold; consequently 1000 grains of aqua regia, made of four parts of muriatic acid and one of nitric, of the strength respectively indicated by the gravities noted above, are capable of dissolving 187 grains of gold. The nitric acid being of no use here but for the oxidation of the muriatic, it is evident, that

The muriatic, the real solvent, should predominate.

* Translated from the Journal de Physique, Feb. 1806, vol. lxii. p. 131.

The

the latter, which is the real solvent of the gold, should predominate in the aqua regia. The same things take place in the solution of platina.

Platina comports itself like gold.

To obtain the muriate easily in a crystallized state, it is advisable to keep an excess of gold in the solution, and to add muriatic acid from time to time, till it is perceived to act on the gold no longer. By proceeding thus the nitric acid is exhausted, and at the end there remains none to disturb the crystallization.

Muriate of gold crystallized.

The solution evaporated to a certain point gives a lamellated crystallization, but a coagulated one if it be concentrated too much. This muriate is so liquefiable, so difficult to obtain dry, and of course without risking considerable loss, that it scarcely ought to be taken out of the retort, if we have no other object in view but to exhibit it. In summer it becomes liquid in the morning, and crystallizes towards evening, and passes through this alternation during the continuance of the hot weather.

Extremely deliquescent.

The taste of the pure muriate is ascerb with a little bitterness, but without that after taste of metal, which render the solutions of silver, copper, &c., so disagreeable.

Acerb and slightly bitter.

The muriate of gold is perfectly soluble in spirit of wine. This solution assisted by heat experiences no change; the alcohol is not converted into ether; distillation separates them, and the muriate is recovered unchanged.

Soluble in alcohol.

This muriate being distilled gives out abundance of water and oxigenated muriatic acid. The gold remains spongy and without lustre at the bottom of the retort. The vapours carry over some of the muriate of gold, which is found in the receiver; but very little, as Boyle observed. The decomposition of the muriate of gold exhibits the same appearances in every respect as that of the muriate of platina: both yield oxigenated acid and pure metal.

Decomposed by heat.

Auriferous Ether.

Sulphuric either takes the muriate from the solution of gold and leaves the nitric acid alone. The crystallized muriate too dissolves in it with the greatest facility, and without residuum.

Sulphuric ether dissolves it,

The auriferous ether on exposure to the air loses its solvent, and is reduced to a yellow, acerb liquid, which is always pure muriate

but will evaporate and leave the muriate unchanged.

C. Hoffmann first mentioned it.

Baumé first proposed gilding with it.

muriate. C. Hoffmann was, I believe, the first who, in his dissertation on the vinous vitriolic acid, made known this action of ether on the solutions of gold. Baumé appears to me to have been the first who proposed the use of the auriferous ether for gilding watch-work. Within these few years it has been announced as well fitted for gilding figures on iron and steel. I shall recite here not my success, but attempts extremely unsuccessful; which render it incumbent on the author of this proposal to explain himself more clearly, if he would render a service to the arts, and to many amateurs, who complain of having lost both their gold and their labour.*

Attempts to gild on steel with it unsuccessful.

The ether that has become coloured by standing on a solution loaded to the highest degree, is far from containing as much gold as is requisite for gilding with success. By means of a siphon with a bulb I drew off the colourless liquid beneath the ether, and replaced it by fresh solution: thus the ether becomes of a deeper colour and more loaded. On the third or fourth change the appearances alter, for the auriferous ether no longer swims at the top, but sinks to the bottom with the weight and consistency of oil of cinnamon: on the contrary, it is the nitric acid that floats, and that must be drawn off by the siphon.

Having at length well saturated the ether, and considering my success as certain, I began to trace letters on polished steel, some with a pen, others with a pencil; the strokes exhibited gold, as might be expected from the application of its muriate to metal so easily decomposed; but I must say, that by no means I could contrive to give them the quantity of gold, or the continuity, consistence, and lustre, that I wished. The gilding was very different from that of Solingen. We shall not be surprized at this, if we analyze the effects of the gilding, for we shall at once discover, that a single stroke of this ether applied on steel immediately produces four different effects, three at least of which are opposite to the end proposed.—To precipitate gold, to precipitate muriate of iron, to lay bare the carbon of the steel, and to take off the polish from every point touched, are the effects produced.

Causes of its failure.

* Prof. Proust certainly had not seen Mr. Stodart's description of the process, Philosophical Journal, vol. xi. p. 215, as appears from the account he gives of his own. T.

It

It occurred to me to plunge the steel into water the moment I had traced the figures, and afterwards to dry it; presuming I should thus diminish the inconveniences arising from the muriate of iron; but the figures did not acquire from this more adhesion, or more lustre. The palm of the hand applied gently to polish them, rubbed them off immediately.

It was to as little purpose that I dried the steel, after it was gilded and washed, with a heat sufficient to burn the hand. The gold indeed was thus rendered more firmly adherent, but friction would not give it more lustre, because, however the ether may be loaded with it, it never deposits enough on the figures to cover the blackness of the metal, and to give that continuity of parts, that consistence, and that reflection of light on which the brilliancy of gold depends. Finally, this gilding, as it is now before me on the plates with which I made my experiments, is not even equal to what may be produced by a solution of sulphate of copper. If such were the results of an ether loaded with gold, and from which I might have promised myself some success, what is to be expected from an auriferous ether prepared according to the common receipts?

Various precipitates of Gold.

Gold precipitated by sulpurated hydrogen, washed and dried, is nothing but a mixture of sulphur and pure gold. Heating it in a retort is sufficient to separate the metal from the sulphur. Consequently there is neither a sulphuret nor a hydrosulphuret of gold. Gold precipitated by sulphurated hydrogen:

The sulphureous acid precipitates it pure. The gold is in such a state of division, that I conceived at first it might be employed for painting on enamel, or for gilding; but the metallic particles were quickly susceptible of an attraction, that collected them together, consolidated them, and made them assume the consistency of a tenacious, though spongy substance. In this state nothing more is to be done with them. —by sulphureous acid:

Of the precipitation of Gold by the sulphate of Iron.

With the solution of this salt we succeed much better, and the result is a fine powder of a purple-red colour, the tint of which, however, is nothing like the purple of Cassius. Being washed in acidulated water to free it from iron, it is to be kept —by sulphate of iron.

 under

under water, because in this state it is proper for experiments that require an easy and prompt solution of this metal.

Gives a deep purple to porcelain.

This gold applied on porcelain gives a deep purple. We shall return to the state of the gold in this colour.

Partly soluble in marine acid.

Marine acid of 12° boiled on this powder of gold, very evidently dissolves some of it, and acquires a yellow colour. A slip of tin put into it produces the purple in an instant. Gold, therefore, assisted by the affinities that favour iron, zinc, &c., is capable of decomposing water. Thus the marine acid, contrary to the received opinion, is capable of attacking gold and silver, as it does so many other metals.

Gold capable of decomposing water.

This precipitate partly soluble in strong nitric acid;

Nitric acid of 40° boiled on this gold dissolves some of it likewise, and becomes coloured.

—but not by that used in parting.

An acid of 36° dissolves some too, but so little, that it is scarcely to be detected by means of tin. With an acid of 32°. like that employed in parting, it may be doubted whether any gold can be taken up; particularly as the cornet is far from exposing so many points to the attack of the acid as the powder in question.

Solution of gold precipitated by phosphorus gas, but not by phosphorus acid.

Hydrophosphorated water precipitates the solution of gold. That in which phosphorus is kept does the same, but the effect is owing exclusively to the gas, for the phosphorus acid has no action on this solution in less than ten or twelve hours.

Precipitation by Alkalis.

Precipitated by potash.

Potash purified by alcohol precipitates from the muriate of gold a powder, that is at first yellow, and then violet, if we operate with a large quantity of water, but which appears black when it has been washed and dried. Nothing is so capricious as this preparation. An excess of alkali, saturation, a boiling heat, are insufficient to render us masters of it. The liquors always remain more or less impregnated with gold.

An uncertain preparation.

It frequently happens, that the precipitation goes on till the next day; but instead of adding to the black powder, it covers it with a metallic pellicle, or even gilds the vessel in a very brilliant manner. I have kept one in this state as an object of curiosity.

A mixture of oxide and reduced gold.

If the black powder have been washed and dried with the gentlest heat, what we should expect to be a pure oxide, is nothing but a mixture of oxide and reduced gold. This is what militates

militates against our being able by these means to ascertain the degree of oxidation of this precious metal.

If muriatic acid be applied to this powder, it dissolves the oxide, and leaves the pure gold, which is always more abundant. Nitric acid of 40° dissolves only a few atoms of the oxide, and it must be assisted by heat. This solution is of a slight yellow colour; and if it be diluted with water, the gold separates from it of the colour of fulminating gold. This precipitate still retains the state of an oxide, accordingly the muriatic acid dissolves it immediately. *Action of acids on it.*

The aqueous sulphuric acid dissolves some of it also, but less than the preceding. It is seen by the violet colour it assumes, if a few drops of muriate of tin be added. To conclude, carbonates are not in any respect more advantageous for the precipitation of gold; which has compelled me to give up this point, repeating the words of Bergman: "all gold is "precipitated with difficulty, so that I am uncertain of the "weights." *Carbonates not preferable to alkalis. Gold, difficult to precipitate.*

Fulminating Gold.

A hundred parts of fulminating gold passed through sulphurated hydrogen, washed and heated, left seventy-three of pure gold. A hundred parts of gold consequently give about 137 of fulminating gold. If any means of appreciating the ammonia that attaches to the oxide could be found, the oxidation of the gold could easily be deduced from this. *Fulminating gold.*

Kunckel observed, that the oxide of gold obtained by means of alkalis, and moistened with ammonia, became fulminating. Orschal too must be reckoned among the number of those who have been near falling victims to its detonation. An agate mortar, in which he was rubbing this dangerous oxide, flew into splinters under his hand. He received no wound, but he adds that he felt a sensation, as if a musket loaded with sand had been discharged full in his face. According to him, Raymond Lully experienced a similar accident. *Dangers attending it.*

Orschal too used fulminating gold to give a purple colour to glass. It even appears, that this use of it was known before his time. Hence it might have been concluded at that period, that tin was not a necessary ingredient in the purple colour. *Used to colour glass purple.*

Changed to purple of Cassius.

Fulminating gold moistened with muriate of tin changes to the purple powder of Cassius, because it parts with oxigen; but the purple powder thrown into ammonia gives very different results, as we shall see further on.

Gold and Mercury.

Muriate of gold with nitrate of mercury at a minimum.

On letting fall a solution of gold, drop by drop, into a solution of nitrate of mercury at a minimum, a violet powder is thrown down, which, according to Orschal, yields a very fine purple. This purple is a mixture of metallic gold and sweet muriate, which may be separated from each other by merely heating them in a retort. On the contrary, if the solution of mercury be poured into that of gold, so that the latter predominates, the precipitate will be found after the space of twelve hours to be pure gold. This arises from the mercurius dulcis being decomposed in its turn. In fact, if mercurius dulcis be kept in a purely muriatic solution of gold, it precipitates the gold, disappears, and is found to be converted into corrosive sublimate.

With nitrate at a maximum.

Nitrate of mercury, the oxide of which is completely at the maximum, likewise precipitates the muriate of gold, but the results are very different, and deserve to be made known.

To understand all that passes in this precipitation, it must be remembered,

1st.—That the oxide of mercury being more powerfully attracted by the muriatic acid than by the nitric, it has always a tendency to quit the latter, in order to unite with the former.

2d.—That the oxide of mercury having a stronger affinity than that of gold for the muriatic acid, the former ought infallibly to displace the latter from this acid.

3d.—That, if the mercury be once saturated with oxigen, it will precipitate the gold with all its oxigen; that is, as it exists in the solution, because in this case it has no motive to take it from the gold.

4th.—That, if the oxide of gold be separated from the muriatic acid by that of mercury, it will fall down, from being insoluble in the nitric acid, which the oxide of mercury has quitted.

These four effects actually take place in the precipitation about to be mentioned.

A solu-

A solution of mercury is first to be prepared by dissolving some red oxide in nitric acid. This is to be diluted with eight or ten times its bulk of pure water, and solution of gold is afterward to be added at different intervals. A yellow precipitate will thus be produced, in colour much resembling fulminating gold. Leave this to subside, draw off the liquid, wash the precipitate several times with boiling water, and dry it in a capsule. Process.

If we go on to exhaust the solution of mercury by continuing to add that of gold, we shall arrive at a point when the latter will render it turbid no longer. The reason of this is obvious: the nitrate of mercury is at length entirely converted into corrosive sublimate; and sublimate, the oxide of which is saturated with oxigen, cannot decompose the muriate of gold.

If the nitrate of mercury employed in this experiment be not completely at the maximum, it is easy to perceive that we should be liable to produce mercurius dulcis, which would then unite with the oxide of gold.

This precipitate is not, as I at first hoped, a pure oxide. It remains mixed or combined with a portion of corrosive sublimate, from which it could not be freed by repeated washings.

A hundred parts of this dry precipitate, heated in a retort, afforded, Component parts of the precipitate.

Water	8,
Corrosive sublimate mixed with mercurius dulcis	16,
Gold	70,
Total	94.

The oxigen fixed in the gold, therefore, amounted to 6; consequently a hundred parts of gold require 8·57 of oxigen to become an oxide. But as these results did not yet afford me any thing satisfactory, I thought it necessary to proceed to a fresh precipitation. The precipitation took place with the same appearances, but the washing was carried much farther than before. The last washings, however, were still rendered turbid by the test of muriate of tin at a minimum; but the products of the distillation, though the same, exhibited themselves very differently. 100 p. of gold oxided by 8·57 of oxigen. Process repeated.

I put

Analysis of the precipitate.

I put a hundred grains of the precipitate into a little glass retort over the flame of a lamp. Scarcely had a few minutes elapsed before a rapid stream of white vapours spouted out, forming a thick cloud, which I took care not to inhale. I judged, that a hasty disoxidation of the gold might have produced this kind of detonation; and in fact the gold was found to be completely disoxided. The retort was coated with mercurius dulcis mixed with corrosive sublimate.

Detonated.

Analysis repeated.

I repeated the distillation, throwing the precipitate by small portions at a time in to a matrass previously weighed and placed over a lamp. The results being less tumultuous than before, allowed me this time to observe better what passed; but it was impossible for me to appreciate the humidity, and consequently the proportion of the oxigen to the gold.

The oxigen in this precipitate, however, was much more abundant, as far as can be conjectured, than in the former; for 100 grains having afforded of

Results.

Mild and corrosive muriate	16,
Gold	58,
Total	74,

it is evident, that, as the 26 wanting to make up the 100 could not contain more than about 8 parts of water, the oxigen must have been in the proportion of 12 parts to 58 of gold; which would give the proportions of gold 100, oxigen 31. I am inclined, however, to consider this as more certain than the preceding; because, as I applied a boiling heat to the first precipitate, part of the gold may have been more or less disoxidated; a circumstance which I took care to avoid in the preparation of the second. But unquestionably it would be premature to attempt to establish any theory on these facts. It is necessary to examine and re-examine them, but my occupations prevent me from doing this at present. I shall only say, that the mercurius dulcis which here accompanies the oxide of gold, did not proceed from any portion of oxide at a maximum, that my mercurial solutions might have retained. What then was the origin of this mercurius dulcis? What could be the occurrence of affinities, that reduced the sublimate to the condition of mercurius dulcis, and united it thus to the oxide of gold?

100 gold to 31 oxigen.

Farther inquiries necessary.

Whence came the mild muriate?

I shall

I shall conclude these details with a property of this precipitate much more extraordinary than those that have been mentioned.

If a few grains be heated on paper over the flame of a candle, they soon melt and explode, emitting their puffs of white smoke before they are reduced to the state of pure gold. But if it be first mixed with a little flowers of sulphur, triturating them together with the point of an ivory spatula, and then heated gently over the candle, it detonates very easily, and with as sharp a noise as fulminating gold.

Extraordinary property of this precipitate.

The first of my two precipitates, in which I suspected there was less oxigen than in the second, detonates notwithstanding as well as this.

The oxide of gold obtained by means of potash, mixed with sulphur and heated in the same manner, melts obscurely, but without the least tendency to detonation. The detonation of the preceding oxides is a constant property that never fails. If the mixture be scattered about, the detonation equally takes place; but when the precipitate is well collected together, the report is single, and consequently very loud. After the detonation nothing is found between the papers but gold in a state of division.

Now if we reflect on a result thus singular, we shall find gold here rendered fulminating by what destroys this property in ammoniacal fulminating gold; and without the assistance of ammonia we bring it to produce effects, the explanation of which must necessarily shake the very theory of fulminating gold. What influence can the two muriates of mercury have in this detonation? This remains to be inquired.

Singularity of this fulminating powder.

(The conclusion in our next.)

XII.

Note from THOMAS YOUNG, *M.D. F.R.S., &c. recommending the Translation of a Memoir of M. Laplace. With some Remarks.*

To

To Mr. NICHOLSON.

Dear Sir,

I take the liberty of recommending for insertion in your Journal, a paper of M. Laplace on "Capillary Tubes," published in the Journal de Physique, for January last; as I imagine it would be satisfactory to your readers to compare it with the "Essay on the Cohesion of Fluids," which you have done me the honour to reprint. As far as M. Laplace has pursued the subject, he has completely confirmed my conclusions, although by a very different mode of calculation: his reasoning appears, however, to me to be defective, for want of attending to the force of repulsion, which in most cases exactly balances that of cohesion. I had contented myself with determining by an approximate construction the form assumed by the surface of a fluid in a cylindrical tube of moderate diameter; I was in hopes that so consummate a mathematician as M. Laplace would have attempted a direct solution of the problem: but he has left it wholly untouched. I was also anxious to find a confirmation of my conclusions respecting the relation between the mutual force of attraction of a solid and a fluid, and the angle formed by their surfaces: but my expectations were again disappointed. The inferences which I had made from the experiments of Taylor, Achard, and Morveau, are such as might have been deduced without much difficulty, either from M. Laplace's theory or from mine: if they had been so fortunate as to attract M. Laplace's attention before his memoir was read to the Institute, he would perhaps have confirmed and extended them with his usual accuracy and ingenuity.

I am, Dear Sir,

Your very obdient humble servant,

THOMAS YOUNG.

Welbeck-street,
28th May, 1806.

Errata.

Page 83, line 15, for .0054 read .054.
—— —— 21, for .1 read 1.
—— 159, —— 21, for .1 read 1.

Abstract

Abstract of a Memoir on the Theory of Capillary Tubes. By M. LAPLACE.*

THIS memoir, destined to appear among those of the first class of the Institute, is preceded by the following analysis of the theory it contains.

The theory of capillary tubes by Clairaut

Clairaut was the first who submitted the phenomena of capillary tubes to strict and accurate analysis, in his *Traité sur la Figure de la Terre.* But his theory, though exposed with the elegance which characterizes that excellent and important work, leaves to be desired a compleat explication of the chief of these phenomena; namely, why the elevation of the fluid above its level in tubes of the same matter, is inversely as their diameters. This great geometer is contented to observe, without proving it, that there must be an infinity of laws of attraction which, when substituted in his formulas, give that result. I have long sought to supply what is wanting in the theory of Clairaut. New researches have at length conducted me not only to ascertain the existence of such laws, but have likewise shewn that every law, according to which the attraction ceases to be perceptible at any perceptible distance, must give an elevation of the fluid in the inverse ratio of the diameter of the tube ; and the result is a complete theory of this description of phenomena.

—imperfect.

Clairaut supposed the capillary attraction extended to the axis of the tube:

Clairaut supposes that the action of the capillary tube is sensible upon the infinitely thin column of the fluid which passes through the axis of the tube. I differ from him in opinion in this respect, and think with Hawksbee, and many other philosophers, that the capillary attraction, like the refractive force and all the chemical affinities, is not sensible, except at imperceptible distances. Hawksbee has observed, that in glass tubes, whether they be very thin or very thick, water rises to the same height whenever the interior diameters are the same. The cylindrical zones of the glass which are at a sensible distance from the interior surface, do not therefore contribute to the ascent of the water, though in each of them separately taken, this fluid would rise above its level. A very simple experiment also proves the truth

But this is not consistent with the facts.

* Read to the French National Institute, 23d Dec. 1805, and translated from the paper communicated by this author to the Journal de Physique, lxii. 120.

That the attraction of a glass tube on water does not act at any perceptible distance is shewn by greasing it.

truth of this principle. If the interior surface of a tube of glass be covered with an extremely thin coating of any greasy substance, the capillary effect will be destroyed as to sense. Nevertheless the tube always acts in the same manner upon the column of fluid in the axis; for the capillary attraction must be transmitted through bodies in the same manner as is observed with regard to gravity and the attractions and repulsions of magnetism, and even of electricity. Newton, Clairaut, and all geometers who have subjected this class of attractions to computation, have proceeded upon that hypothesis: since therefore the capillary attraction is destroyed by the interposition of a coating of fat matter, however thin it may be, it must follow that the action of the tube will be insensible at any sensible distance.

Mercury attracts and adheres to a glass tube; but in general it is repelled by a thin coating of water.

The following phenomenon affords an additional proof of the principle here announced. It is known that by strong ebullition of mercury in a capillary tube the fluid becomes elevated to the level, and even above the level if the boiling be continued. This phenomenon appears to me to depend upon the thin coating of water which in the ordinary state lines the inner surface of the tube and weakens the mutual action of the glass and the mercury; an action which becomes more and more manifest in proportion as the thickness of that coating is diminished by the heat of boiling. In the experiments which I made with M. Lavoisier upon barometers by boiling the mercury for a long time in them, we caused the convexity of its interior surface to disappear; we even succeeded in rendering it concave; but we always restored the effect of the capillarity by introducing a drop of water into the tube. If we consider the extreme thinness which the aqueous coat must have, particularly when the tube and the mercury has been well dried, which process is not sufficient to destroy the capillarity, we may form a judgment that the action of the glass on this fluid is not sensible but at insensible distances.

The author's process of analysis, or the theory of capillary tubes.

Proceeding on this principle, I determine by the formulas of my treatise de Mécanique Céleste, the action of a mass of fluid terminated by a concave or convex spherical surface upon an interior column of fluid included in an infinitely narrow canal which passes through the axis of that surface. By this action I understand the pressure which the fluid included in the canal

canal would exercise, by virtue of the attraction of the entire mass, upon a plane base situated in the interior of the canal, perpendicular to its sides, at any sensible distance whatever from the surface, that base being taken for unity. I show that this action is less or greater than if the surface were plain; less if the surface be concave; greater if the surface be convex. Its analytical expression is composed of two terms; the first much greater than the second, expresses the action of the mass terminated by a plain surface, and I think that on this term depend the phenomena of the adherence of bodies to each other, and of the suspension of the mercury in a barometer tube, at an elevation which is three or four times greater than would arise from the pressure of the atmosphere. The second term expresses that part of the action which is due to the sphericity of its surface: it is positive or negative, accordingly as the surface is convex or concave. I shew that in each case this term is in the inverse ratio of the radius of the spherical surface. Thence I conclude the general theorem, namely, that in all the laws wherein the attraction is not sensible but at insensible distances, the action of a body, terminated by a curve surface, on an interior canal infinitely narrow and perpendicular to that surface in any point whatever, is equal to half the sum of the actions on the same canal, of two spheres which should have for their radii the greatest and the smallest of the radii osculators of the surface at that point. By means of this theorem and the laws of the equilibrium of fluids, we may determine the figure which a fluid mass animated by gravity or weight must take. I shew that in a cylindrical tube of considerable diameter, the section of the surface of the fluid, by a vertical plane, is a curve of the genus of those which geometers have called elastic, and which are formed by an elastic plate or blade bended by weights; this results from the circumstance, that in that section, as in the elastic curve, the force due to the curvature is reciprocal to the radius osculator. If the tube be very narrow, the surface of the fluid approaches the more to that of a spherical segment as the diameter of the tube is smaller. I afterwards prove, that in different tubes of the same matter, these segments are nearly alike; whence it follows that the radii of their surfaces are very nearly proportional to the diameters of the tubes. This similitude of

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

the spherical segments will be evident, if we consider that the distance where the action of the tube ceases to be sensible, is imperceptible; so that if, by means of a very powerful microscope, we could succeed in rendering it sensible to the amount of one *millimetre* (or one twenty-fifth of an inch English), it is probable that the same amplifying power would give to the diameter of the tube an apparent magnitude of several metres. The surface of the tube may therefore be considered as being very nearly plain in a radius equal to that distance; the fluid in that interval will therefore fall or rise as to that surface very nearly as if it were plain: beyond that space, the fluid being no longer subjected as to sense to any power but that of weight and its own action upon itself, its surface will be extremely near to that of a spherical segment, of which the extreme sides, being those of the surface at the limits of the sphere of sensible activity of the tube, will be very nearly alike inclined to the horizon in the different tubes; whence it follows that all these segments will be very nearly similar.

The near coincidence of these results gives the true cause of the ascent or depression of fluids in capillary tubes in the inverse ratio of their diameters. If through the axis of a tube of glass, we imagine a canal infinitely narrow, which being recurved a little below the tube, shall proceed to terminate at the plane and horizontal surface of the water of a vessel in which the lower extremity of the tube is plunged, the action of the water of the tube on this canal will be less, on account of the concavity of its surface, than the action of the water of the vessel on the the same canal; the fluid must therefore rise in the tube to compensate this difference; and as this is from what has been shewn in the inverse ratio of the diameter of the tube, the elevation of the tube above its level must follow the same ratio.

If the fluid be mercury, its surface within a capillary tube of glass is convex; its action on the canal is therefore stronger than that of the mercury of the vessel, and the fluid must be depressed in the tube on account of this difference, and consequently in the inverse ratio of the diameter of the tube.

The attraction of capillary tubes has not, therefore, any influence on the elevation or depression of the fluids which they include, except by determining the inclination of the first planes of

of the surface of the interior fluid, which are extremely near the sides of the tube, upon which inclination the concavity or convexity of that surface and the length of its radius depend. If by the effect of rubbing the interior fluid against the sides of the tube the curvature be increased or diminished, the capillary effect will increase or diminish in the same proportion.

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

It is interesting to ascertain the radius of curvature of the surface of water included in capillary tubes of glass. This may be known by a curious experiment, which shows at the same time the effects of the concavity and convexity of surfaces. It consists in plunging in water to a known depth a capillary tube, of which the diameter is likewise known. The lower extremity of the tube is then to be closed with the finger, and the tube being taken out of the water, its external surface must be gently wiped. Upon withdrawing the finger in this last situation, the water is seen to subside in the tube and form a drop at its lower base; but the height of the column is always greater than the elevation of the water in the tube above the level in the common experiment of plunging it in water. This excess in the height is owing to the action of the drop upon the column, on account of its convexity; and it is observable that the increase in the elevation of the water is more considerable the smaller the diameter of the drop beneath. The length of the fluid column which came out by subsidence to form the drop, determines its mass; and as its surface is spherical, as well as that of the interior fluid, if we know the height of the fluid above the summit of the drop, and the distance of this summit from the plane of the interior base of the tube, it will be easy to deduce the radii of these two surfaces. Some experiments lead me to conclude that the surface of the interior fluid approaches very nearly to the figure of an hemisphere.

Clairaut has made this singular remark, namely, that if the law of attraction of the matter of the tube upon the fluid differs only by its intensity from the law of the attraction of the fluid upon itself, the fluid will be elevated above the level, while the intensity of the former of these attractions exceeds the half of the intensity of the latter. If it be exactly the half, it is easy to show that the surface of the fluid in the tube will be horizontal, and that it will not be elevated above the level. If these

two

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

two intensities be equal, the surface of the fluid in the tube will be concave and hemispherical, and the fluid will rise above the level. If the intensity of the attraction of the tube be nothing, or insensible, the surface of the fluid in the tube will be convex and hemispherical, and the fluid will be depressed below the level. Between these two limits, the surface of the fluid will be that of a shperical segment, and it will be concave or convex accordingly as the intensity of the attraction of the matter of the tube upon the fluid shall be greater or less than the half of that of the attraction of the fluid upon itself.

If the intensity of the attraction of the tube upon the fluid surpass that of the attraction of the fluid upon itself, it appears probable to me that the fluid in that case, attaching itself to the tube, will form an interior tube, which alone will raise the fluid, of which the surface will be concave and hemispherical. I presume that this is the case with water in a tube of glass.

After having considered fluids terminated by spherical surfaces, I consider them as terminated by cylindrical surfaces. This case is that of a fluid included between two planes very near each other, and having their lower extremities plunged in a vessel containing the same fluid. I find by analysis that the fluid ought to rise or be depressed accordingly as the cylindric surface of the fluid is concave or convex, and that this elevation or depression also follows the inverse ratio of the distance between the planes. I find also that the elevation or depression is equal to that which would take place in a cylindrical tube of which the internal semi-diameter should be equal to that distance. Having obtained this result by analysis, I proposed to M. Hauy to verify the same by experiments, and he found it perfectly conformable to those which at my request he made. And afterwards, upon revising what has been written on the capillary action, I saw that these experiments had already been made with great care in the presence of the Royal Society of London, under the eyes of Newton, and that their result is exactly conformable to that of the analysis. This may be seen by the following passage in his Optics, an admirable work, in which that profound genius has anticipated beyond the times in which he lived, a great number of original views which have been confirmed by modern chemistry. "And of the same kind" (says he, question 31.) "with these experiments are those which

"follow

"follow: If two plane polished plates of glass (suppose two "pieces of a polished looking-glass) be laid together, so that "their sides be parallel and at a very small distance from one "another, and their lower edges be dipped into water, the "water will rise up between them. And the less the distance "of the glasses is, the greater will be the height to which the "water will rise. If the distance be about the hundredth part "of an inch, the water will rise to the height of about an inch; "and if the distance be greater or less in any proportion, the "height will be reciprocally proportional to the distance very "nearly. * * * * * . And if slender pipes of glass be dip-"ped at one end into stagnating water, the water will rise up "within the pipe, and the height to which it rises will be re-"ciprocally proportional to the diameter of the cavity of the "pipe, and will equal the height to which it rises between two "panes of glass, if the semi-diameter of the cavity of the pipe be "equal to the distance between the planes or thereabouts. And "these experiments succeed after the same manner in vacuo as "in the open air, (as hath been tried before the Royal Socie-"ty), and therefore are not influenced by the weight or pres-"sure of the atmosphere."

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

The capillary phenomena of inclined planes and of conical and prismatic tubes are so many corollaries of my analysis. Thus it is observed that a short column of water in a conical tube, open at both ends and kept horizontal, is moved towards the summit of the tube; and from what has been explained, it is clear that this must be the event. In fact, the surface of the fluid column is concave at its two extremities; but the radius of its surface is smaller at the end next the summit than at the end next the base; the action of the fluid on itself is therefore less on the side next the summit, and consequently the column must tend that way. But if the column of fluid be mercury, its surface will then be convex, and its radius also less towards the summit than towards the base; but on account of its convexity, the action of the fluid on itself is greater towards the summit, and the column must be carried towards the base of the tube.

We may balance this action by the proper weight of the column, and keep it suspended in equilibrio, by inclining the axis of the tube to the horizon. A very simple calculation shews that

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

that if the length of the column be very small, the sine of the inclination of the axis will then be very nearly in the inverse ratio of the square of the distance of the middle of the column from the summit of the cone; and this also takes place, if instead of causing a drop of the fluid to move in a conical tube it be made to move between two planes forming a very small angle between them. These results are entirely conformable to experience, as may be seen in the Optics of Newton (query 31.).

Calculation also teaches us, that the sine of the inclination of the axis of the cone to the horizon, is then, very nearly, equal to a fraction, of which the denominator is the distance of the middle of the drop from the summit of the cone, and its numerator the height to which the fluid would rise in a cylindrical tube, having for its diameter that of the cone at the middle of the column. If two planes which include a drop of the same fluid form between them an angle equal to double the angle formed by the axis of the cone and its sides, the inclination to the horizon of a line which equally divides the angle formed by the planes, need be only half that required in the axis of the cone, in order that the drop should remain in equilibrio.

The preceeding theory likewise gives the explanation and measure of a singular phenomenon, presented by experiment. Whether the fluid be elevated or depressed between two vertical planes, parallel to each other, and plunged in the fluid at their lower extremities, their planes tend to come together. Analysis shews us that if the fluid be raised between them, each plane will undergo, from without, inwards, a pressure equal to that of a column of the same fluid, of which the height would be half the sum of the elevations, above the level, of the points of contact of the interior and exterior surfaces of the fluid with the plane, and of which the base should be the parts of the plane comprised between the two horizontal lines drawn through those points. If the fluid be depressed between the planes, each of them will in like manner undergo from without, inwards, a pressure equal to that of a column of the same fluid, of which the height would be half the sum of the depressions below the level of the points of contact of the interior and exterior surfaces of the fluid with the plane,

and

and of which the base should be the part of the plane comprized between the two horizontal lines drawn through those points.

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

In general, if we compare the theory which I expose, to the numerous experiments of philosophers on the capillary action, we shall see that the results obtained by those experiments are deducible not by vague and uncertain considerations, but by a train of geometrical reasoning, which appears to me to leave no doubt of the truth of this theory. I am desirous that this application of analysis to one of the most curious objects of natural philosophy may prove interesting to geometers, and excite them to multiply more and more these applications, which unite the advantage of giving certainty to physical theories, and adding to the perfection of the analytical art, by the frequent demand for new artifices of calculation.

Note (by the Author).

The demonstration of the preceding theories will be published in one of the succeeding volumes of the Institute. The following results of analysis may serve to direct those who may be disposed to deduce the principal themselves.

Let us denote by $\phi\ (f)$ the law of the attraction of a fluid particle upon another particle, placed at the distance f; $\phi\ (f)$ decreasing with an extreme rapidity, while f increases, and being insensible for every sensible value of f. Let us also designate by $c-\Pi\ (f)$ the integral $\int df.\ \phi\ (f)$ taken from $f=o$, c being the value of that integral, when f is infinite; $\Pi\ (f)$ will in like manner decrease with an extreme rapidity, and will be also insensible for all the sensible values of f. Let us also denote by $c'-\Psi\ (f)$ the integral $\int f\, df.\ \Pi\ (f)$, c' being its value when f is infinite; $\Psi\ (f)$ will be likewise insensible for all the sensible values of f. Lastly, let us denote by K and H the integrals $2\ \pi \int d\,z.\ \Psi\ (z)$ and $2\ \pi \int z\, d\,z.\ \Psi\ (z)$ taken from z nul to z infinite, π being the semi-circumference of which the radius is unity. It will be seen by the analysis of No. 12, of the second book of La Mécanique Céleste, that the action of a sphere of which the radius is b, upon the fluid included in a canal infinitely narrow, perpendicular to its surface, is $K+\frac{H}{b}$. By this action I understand the pressure which the fluid of the canal would exert by virtue of this action, upon a base perpendicular to the direction of the canal, placed in its interior at any sensible distance whatever, from the surface of the body, and taken for unity. This would also be the expression of the action of a body terminated by a sensible segment of a sphere whose radius is b; which results

Laplace's theory of the elevation and depression of fluids by that effect of attraction which is called capillary.

from the consideration that the attraction is not sensible but at insensible distances. If the surface, instead of being convex, is concave, b must be made negative, and then the action becomes $K - \frac{H}{b}$. In the case of a plane or where b is infinite, it becomes reduced to K.

These attractions are of the same description as those on which the refraction of light depends, and which I have considered in the Nos. 2 and 3, of the second book of my Mécanique Céleste. That which renders them independent of the dimensions of bodies, is that it is indifferent to take the preceding integrals, from zero to infinite, or from zero to a sensible value of the variable quantity.

The theorem relative to the action of any body whatever, upon an interior canal, infinitely narrow and perpendicular to its surface, is demonstrated by observing that at each point of the surface, we may conceive an osculator ellipsoid which confounds itself with the body, so that the difference of the actions of these two bodies upon the canal is insensible; and it is easy to prove that the action of an ellipsoid upon a canal which passes through one of its axes, is equal to the half sum of the actions of two spheres, which should have for their radii the greatest and the smallest of the radii osculators of the surface of the ellipsoid at the extremity of that axis. Calling, therefore, these two radii, b, and b', the action of the body will be $K + \frac{H}{2}. (\frac{1}{b} + \frac{1}{b'})$ In the case of a cylindrical surface b is infinite, and the action becomes $K + \frac{H}{2b'}$. The difference between this action and that of a body terminated by a plane surface is therefore $\frac{H}{2b'}$, and consequently the half of what it would be if the surface of the body were spherical and of a radius equal to b'. This is the reason why a fluid is raised or depressed between two parallel planes, only half as much as in a cylindrical tube equal in diameter to the measure of their distance.

XIII.

Processes for making cheap and durable Paints with Fish-Oil. By MR. THOMAS VANHERMAN, *No 21, Mary-le-bone Street, Golden Square.**

Advantageous colours in oil.

HAVING applied a great portion of my time, for several years past, to discover a method of preparing a cheap and durable

* Addressed to the Society of Arts, who awarded him the silver medal and twenty guineas. See their Transactions for 1805.

rable composition for the defence and preservation of all work exposed to the inclemency of the weather, I have now the satisfaction of laying before the Society for the Encouragement of Arts, &c. specimens of some of the above colours ready prepared for use, which will, I flatter myself, be found superior to all others, for cheapness and durability, equal to any in beauty, and not subject to blister or peel off by the sun.

The vehicle made use of for the said paints is fish-oil, the preparation of which is so simple, that when known, gentlemen who have large concerns to paint, may have this composition of any colour manufactured, and laid on by their labourers. I have sent a bottle of the prepared oil; also, a number of patterns, of various colours. The highest price of any, does not exceed three-pence per pound, and many of them so low as two-pence, in a state fit for use. I have likewise sent a pot of white-lead which has been ground with prepared fish-oil, and which, when thinned with linseed-oil, surpasses any white hitherto made use of for resisting all weathers, and retaining its whiteness. I hope my humble endeavours will merit the approbation of the Society, before whom, I will, at any time they shall please to appoint, make the various experiments they may require.

The vehicle is fish-oil.

—and is very cheap.

Instructions how to refine one ton of Cod, Whale, or Seal Oil, for painting, with the cost attending it.

Refining of fish-oil.

	£.	s.	d.
One ton of fish-oil, or 252 gallons,	36	0	0
32 gallons of vinegar, at 2s. per gallon,	3	4	0
12lbs. litharge, at 5d. per lb.	0	5	0
12lbs. white copperas, at 6d. ditto,	0	6	0
12 gallons of linseed-oil, at 4s. 6d. per gallon,	2	14	0
2 gallons of spirits of turpentine, at 8s. ditto,	0	16	0
	£43	5	0

252 gallons of fish-oil,
12 ditto linseed-oil,
2 ditto spirits of turpentine,
32 ditto vinegar,

298 gallons, worth 4s. 6d. per gallon,

 Which

Which produces ··	£67	1	0
Deduct the expense··	43	5	0
	£23	16	0 profit.

To prepare the Vinegar for the Oil.

Vinegar with litharge and sulphate of zinc, is agitated and left to stand with fish-oil. It thus becomes fit for painters' colours.

Into a cask which will contain about forty gallons, put thirty-two gallons of good common vinegar; add to this twelve pounds of litharge, and twelve pounds of white copperas in powder; bung up the vessel, and shake and roll it well twice a day for a week; when it will be fit to put into a ton of whale, cod, or seal oil; (but the Southern whale oil is to be preferred, on account of its good colour, and little or no smell) shake and mix all together, when it may settle until the next day; then pour off the clear, which will be about seven-eights of the whole. To this clear part add twelve gallons of linseed-oil, and two gallons of spirits of turpentine; shake them well together, and after the whole has settled two or three days, it will be fit to grind white-lead, and all fine colours in; and, when ground, cannot be distinguished from those ground in linseed-oil, unless by the superiority of its colour.

If the oil is wanted only for coarse purposes, the linseed-oil and oil of turpentine may be added at the same time that the prepared vinegar is put in, and after being well shaken up, is fit for immediate use without being suffered to settle.

The vinegar is to dissolve the litharge, and the copperas accelerates the dissolution, and strengthens the drying quality.

The residue, or bottom, when settled, by the addition of half its quantity of fresh lime-water, forms an excellent oil for mixing with all the coarse paints for preserving outside work.

Note.—All colours ground in the above oil, and used for inside work, must be thinned with linseed-oil and oil of turpentine.

☞ The oil mixed with lime-water, I call *incorporated oil.*

The

Painters' colours cheaply and well prepared with fish-oil.

The method of preparing and the expence of the various Impenetrable Paints.

First.—*Subdued Green.*

	£.	s.	d.
Fresh lime-water, 6 gallons	0	0	3
Road dirt finely sifted, 112 pounds,	0	1	0
Whiting, 112 ditto,	0	2	4
Blue-black, 30 ditto,	0	2	6
Wet blue, 20 ditto,	0	10	0
Residue of the oil, 3 gallons,	0	6	0
Yellow ochre in powder, 24 pounds,	0	2	0
	£1	4	1

This composition will weigh 368 pounds, which is scarce one penny per pound. To render the above paint fit for use, to every eight pounds add one quart of the incorporated oil, and one quart of linseed-oil, and it will be found a paint with every requisite quality, both of beauty, durability, and cheapness, and in this state of preparation does not exceed two-pence-halfpenny per pound; whereas the coal tar of the same colour is six-pence.

The method of mixing the ingredients for the Subdued Green.

First, pour six gallons of lime-water into a large tub, then throw in 112 pounds of whiting; stir it round well with a stirrer, let it settle for about an hour, and stir it again. Now you may put in the 112 pounds of road dirt, mix it well, then add the blue-black, after which the yellow ochre, and when all is tolerably blended, take it out of the tub and put it on a large board or platform, and with a labourer's shovel mix, and work it about as they do mortar. Now add the wet blue, which must be previously ground in the incorporated oil (as it will not grind or mix with any other oil). When this is added to the mass, you may begin to thin it with the incorporated oil in the proportion of one quart to every eight pounds, and then the linseed-oil in the same proportion, and it is ready to be put into casks for use.

Lead

Painters' colours cheaply and well prepared with fish-oil.

Lead Colour.

	£.	s.	d.
Whiting, 112 pounds,	0	2	4
Blue-black, 5 ditto,	0	1	8
Lead ground in oil, 28 ditto,	0	14	0
Road dirt, 56 ditto,	0	0	6
Lime-water, 5 gallons,	0	0	6
Residue of the oil, 2½ ditto,	0	5	0
Weighs 256 lbs.	£1	4	0

To the above add two gallons of the incorporated oil, and two gallons of linseed-oil to thin it for use, and it will not exceed 1¾*d.* per pound.

Note.—The lime-water, whiting, road dirt, and blue-black, must be first mixed together, then add the ground lead, first blending it with two gallons and a half of the prepared fish-oil, after which thin the whole with the two gallons of linseed-oil, and two gallons of incorporated oil, and it will be fit for use. For garden doors, and other work liable to be in constant use, a little spirits of turpentine may be added to the paint whilst laying on, which will have the desired effect.

Bright Green.

	£.	s.	d.
112 pounds yellow ochre in powder, at 2d. per lb.	0	18	8
168 ditto road dust,	0	1	8
112 ditto wet blue, at 6d. per lb.	2	16	0
10 ditto blue-black, at 3d. ditto,	0	2	6
6 gallons of lime-water,	0	0	6
4 ditto fish-oil prepared,	0	12	0
7½ ditto incorporated oil,	0	15	0
7½ ditto linseed-oil, at 4s. 6d. per gallon,	2	8	9
592lbs. weight.	£7	15	1

This excellent bright green does not exceed three-pence farthing per pound ready to lay on, and the inventor challenges any colour-man or painter, to produce a green equal to it for eighteen-pence.

Afte

After painting, the colour left in the pot may be covered with water to prevent it from skinning, and the brushes, as usual, should be cleaned with the painting knife, and kept under water.

Painters' colours cheaply and well prepared with fish-oil.

A brighter green may be formed by omitting the blue-black; and

A lighter green may be made by the addition of ten pounds of ground white-lead.

A variety of greens may be obtained, by varying the proportions of the blue and yellow.

Observe that the wet blue must be ground with the incorporated oil, preparatory to its being mixed with the mass.

Stone Colour.

	£.	s.	d.
Lime-water, 4 gallons,	0	0	4
Whiting, 112 pounds.	0	2	4
White-lead ground, 28 pounds, at 6d. per lb.	0	14	0
Road dust, 56 pounds,	0	0	6
Prepared fish-oil, 2 gallons,	0	6	0
Incorporated oil, 3½ gallons,	0	7	0
Linseed-oil, 3½ ditto,	0	15	9
Weighs 293lbs.	£2	5	11

The above stone colour, fit for use, is not two-pence per pound.

Brown Red.

	£.	s.	d.
Lime-water, 8 gallons,	0	0	8
Spanish brown, 112lbs.	1	0	0
Road dust, 224lbs.	0	2	0
4 gallons of fish-oil,	0	12	0
4 ditto incorporated oil,	0	8	0
4 ditto linseed-oil,	0	18	0
Weighs 501lbs.	£2	0	8

This most excellent paint is scarcely one penny per pound. The Spanish brown must be in powder.

A good

Painters' colours cheaply and well prepared with fish-oil.

A good chocolate colour is made by the addition of blue-black in powder or lamp-black, till the colour is to your mind, and a lighter brown may be formed by adding ground white-lead.

Note.—By ground lead, is meant white-lead ground in oil.

Yellow is prepared with yellow ochre in powder, in the same proportion as the Spanish brown.

Black is also prepared in the same proportion, using lamp-black or blue-black.

To whiten Linseed-Oil.

Take any quantity of linseed-oil, and to every gallon add two ounces of litharge; shake it up every day for fourteen days, then let it settle a day or two; pour off the clear into shallow pans, the same as dripping-pans, first putting half a pint of spirits of turpentine to each gallon. Place it in the sun, and in three days it will be as white as nut-oil.

This oil, before it is bleached, and without the turpentine, is far superior to the best boiled oil, there being no waste or offensive smell.

THOMAS VANHERMAN.

From experiments made, it appears that fine sand will not answer the purpose of road dirt in painting, and that this dry dirt or dust collected in highways much travelled by horses and carriages, and afterwards finely sifted, is the article recommended, as possessing the properties required.

Enclosed you will find a letter from Mr. Hill, West Lavant, Sussex, builder, and surveyor to his Grace the Duke of Richmond, with his opinion respecting the painting of his Grace's house and premises, at Earl's Court, Little Chelsea; which was finished December, 1803.

The Letter.

SIR,

I have just received your letter dated the 15th instant, and am happy to find that your oil and colour business so well stands the test of others, as well as that of myself. The fish-oil composition you made use of, in all the painting you have done

done at Earl's Court, Kensington, for his Grace the Duke of Richmond, under my superintendance, in 1802-3, was fully equal, if not superior to any painting done in the usual way with linseed-oil, white-lead, &c. I have also the highest opinion of your coarse composition and fish-oil you made use of on the out-buildings, fences, &c. on the above premises; the great body and hard surface it holds out, must be of the greatest preservation to all timbers and fences, exposed to open air, and all weathers. It must also be of the greatest service on plastered stucco, external walls, &c.

Painter's colours cheaply and well prepared with fish-oil.

If any father attestation from me, relative to the business you did at the above premises, can be of any service to you, you will command,

Sir, your obedient servant,

West Lavant, W. HILL.
Feb. 7th, 1805.

I beg leave here to subjoin a receipt for a constant white for the inside painting of houses; which paint, though not divested of smell in the operation, will become dry in four hours, and all smell gone in that time.

White Paint.

To one gallon of spirits of turpentine, add two pounds of frankincense, let it simmer over a clear fire until dissolved; strain it and bottle it for use. To one gallon of my bleached linseed-oil, add one quart of the above, shake them well together and bottle it also. Let any quantity of white-lead be ground with spirits of turpentine very fine, then add a sufficient portion of the last mixture to it, until you find it fit for laying on. If in working it grows thick, it must be thinned with spirits of turpentine.—It is a flat or dead white.

XIV.

Letter on the Properties of Tempered Steel. From a Correspondent. T. B.

To Mr. NICHOLSON.

SIR,

IN one of your Journals, I do not recollect which, you signified

Intersting properties of tempered steel.

signified your intention of giving in a future number, some ideas upon certain singular properties of tempered steel. A number of unexplained facts have for some time been known to the workers of steel-plate. As I am concerned in a manufactory of the kind, and in the daily habit of witnessing those curious and anomalous appearances, I thought you might in some measure profit by the following description of the changes which take place in the various processes of hardening, tempering, hammering, burnishing, &c.

Steel-plate hardened and then reduced to spring temper, lost its elasticity by hammering, grinding, &c.

I took a steel plate 30 inches long, 12 broad, and about ,04 thick; I hardened it in a composition of oil and tallow, and afterwards tempered it down to a spring temper; it was now so elastic as to recover its position after being bended; by hammering it to set it straight, it lost a part of its elasticity; after being ground in the same manner as a saw, the elasticity became still less, having nearly returned to the same state as before hardened; it was then very uniformly heated 'till it became blue, it now recovered the whole of its elasticity; after being glazed bright upon a glazier coated with emery, the elasticity was found to be impaired, but in a less degree than when it was ground; the same effect was also produced by rubbing with emery or sand-paper, and also by burnishing; invariably the elasticity was recovered by bluing, and hence this is always the last operation in the manufactory of elastic steel-plate. Should you at some future opportunity favour the public with your opinion on this subject, and these hints have in the least assisted your inquiry, it will be the utmost wish of

—but recovered the whole spring by bluing.

Your humble and obedient servant,

Sheffield, T. B.
June 18, 1806.

XV.

Description of a process for clearing Feathers from their Animal Oil. By Mrs. Jane Richardson.*

Feathers are soaked in lime-water and afterwards drained, washed, and dried.

TAKE for every gallon of clean water, one pound of quicklime; mix them well together, and when the undissolved lime is

* For which the Society of Arts awarded twenty guineas. From their Transactions, 1805. The attestations were very satifactory.

is precipitated in fine powder, pour off the clear lime-water for use, at the time it is wanted.

Put the feathers to be cleaned in another tub, and add to them a quantity of the clear lime-water, sufficient to cover the feathers about three inches, when well immersed and stirred about therein.

The feathers, when thoroughly moistened, will sink down and should remain in the lime-water three or four days, after which the foul liquor should be separated from the feathers by laying them on a sieve.

The feathers should be afterwards well washed in clean water and dried upon nets; the meshes about the fineness of cabbage-nets.

The feathers must from time to time be shaken upon the nets, and as they dry will fall through the meshes, and are to be collected for use.

The admission of air will be serviceable in the drying; the whole process will be completed in about three weeks; after being prepared as above mentioned, they will only require beating for use.

XVI.

Note from H. DAVY, ESQ. *F. R. S., &c. &c, on the Fluoric Acid in Wavellite.*

To Mr. NICHOLSON,

DEAR SIR, *Killarney, Ireland, June* 15.

I SHALL feel much obliged to you, if amongst the articles of intelligence in your Philosophical Journal, you would mention that I have found the acid which exists in minute quantity in the wavellite (the new fossile from Barnstable) to be the fluoric acid, in such a peculiar state of combination as not to be rendered sensible by sulphuric acid. Fluoric acid in wavellite.

I am, Dear Sir,
with great respect,
Your obedient servant,
H. DAVY.

W. Nicholson, Esq.

XVII.

SCIENTIFIC NEWS.

Prospectus of an Establishment to be called the London Chemical Society.

Chemical society.

IT has been observed, that those who cultivate any particular branch of experimental science are solicitous of associating with others engaged in similar studies; a common interest in the same subject of conversation excites a spirit of inquiry; thought gives rise to thought, and new ideas, collected in the friendly intercourse of society, often lead to investigations of the greatest importance. The student finds many difficulties removed which impede his progress by the ready information he obtains from men of higher acquirements, whilst those, who are skilled in chemical pursuits, frequently receive important observations from the mere lover of the science: to this may be added, that men, however great their learning or ardour may be for any particular branch of inquiry, yet, when deprived of the opportunity of communicating their ideas to others, not only become negligent and uninterested in improving the stock of knowledge they already possess, but are seldom solicitous about the further cultivation of their mental powers.

From a conviction of these truths, a number of gentlemen, who have a taste for philosophical chemistry are determined to form themselves into a society, in which the talents of a number may be united, and become extensively useful to each other, by mutual communication of their views, their labours, and acquisitions. That their endeavour may prove as interesting as possible, particularly to those promoters of chemical science who cannot devote much time to the perusal of literary journals and periodical publications, arrangements will be made to collect, as speedily as possible, all the chemical news which issue from the laboratories of other operators, both at home and on the continent; and correspondences will be established

tablished to obtain the earliest and best information respecting whatever shall offer itself as new and important in the departments of chemistry, of natural philosophy, and the arts and manufactures, which are dependent on these branches of knowledge. To keep pace with the existing state of chemical science, the intelligence thus collected shall be regularly detailed in their respective meetings; and a book of reference kept as a register, containing the growing mass of philosophical information, which will be laid on the table for the use of the members; together with all those publications and academical journals of repute, which exhibit the transactions of ingenious men in every part of the world. Chemical society.

The views of this society however will not be confined to the mere detail of literary intelligence and chemical conversations; a principal part of their labour will devolve to the practical department of the laboratory. To accomplish this as perfectly as possible, all the interesting discoveries, which from time to time enrich the domain of chemistry, and particularly those complicated, expensive, and difficult experiments, which can be repeated by few individuals only, shall be exhibited in their own laboratory; being persuaded, that important experimental inquiries, when once witnessed, seldom fail to excite that degree of ardour which gives increasing energy to scientific research.

From this the Chemical Society will direct their attention to all such original and specific experiments, as may individually be proposed, and the results they afford shall be minuted in the journal of the laboratory, kept for that purpose, and afterwards published in such a manner as may be directed. These inquiries will embrace whatever is deemed worthy of experimental research in the extensive departments of philosophical, practical, and technical chemistry. It is perhaps needless to state, that their laboratory will be open for the analysis of ores, soils, manures, and such substances in general as are found in the British dominions, and are deemed of private or public importance.

And, as it is certain that the progress, as well as the accurate and extensive ideas, which the cultivators of chemical science may acquire, are greatly facilitated and promoted, by attending to the manipulations, and processes of the practical chemist;

Chemical society.

chemist; it is likewise intended, that all the multifarious operations of the laboratory, shall be regularly employed for obtaining from the crude materials of nature, all those substances which the society requires as instruments of research, or as specimens of truths, as well as those articles used in the chemical arts, and by manufacturers and artists. This part of the views of the Chemical Society will constitute a perpetual series of operations, well calculated to exhibit a summary exposition of all the general and particular processes of the scientific laboratory: a consideration highly important to the progress of real improvement.

To give effect to this undertaking, a regular laboratory is already fitted up, and an extensive collection of apparatus and instruments will be procured, to ensure those auxiliary advantages which are essential to the pursuit of the science.

Such are the outlines of the plan to which the views of the Chemical Society will be directed. A more particular detail of rules and proceedings would be premature and superfluous. It must nevertheless be remarked, that whatever encouragement the establishment may receive, the admission of subscribers, is for the present limited to sixty, and the annual subscription fixed at three guineas.

An unlimited number of gentlemen residing in the country may be admitted as subscribers, on paying one guiuea annually, which shall entitle them to visit the society as members, whenever they reside in the capital, provided their stay in town does not exceed three months.

After the first meeting the admission of members shall be decided by ballot, and those who are not inclined to adopt the regulations, then agreed upon by the majority of the subscribers, shall have their subscription immediately returned.

A code of laws will be formed, and proper officers elected so as to form a regular society, which shall be denominated the London Chemical Society.

The admission of members is for the present confined to a committee, who on the present occasion, address the chemical public, and request, that such gentlemen as are desirous of becoming subscribers may favour them with their names, for which purpose a book is opened at their laboratory, *No.* 11, *Old Compton Street, Soho.*

Ancient

*Ancient Works in America, resembling Fortifications.**

The artificial works, best known by the name of fortifications, are daily discovered, in great numbers, and many of them of vast extent, in various parts of the United-States, particularly in the fertile countries adjacent to the rivers Ohio and Missisippi, and their branches. In some of the tumuli, or barrows, connected with these works, copper implements, of different kinds, have been found. So that there can be no doubt that the people who formed, or who possessed, these works, were acquainted with the use of copper. But how far this metal was in *general* use among them, we are not yet prepared to determine. This point, however, may be determined, at some future period. Ancient works in America.

Bishop Madison's ingenious speculations concerning the *design* of the works alluded to,† have induced some persons to suppose, that they were never intended to serve the purposes of fortifications. But for whatever purposes they were used, it is certain, that these works could never have been constructed by a people in the state of society in which the Europeans found the Indian inhabitants of the tracts of country in which the supposed fortifications are so abundantly distributed: and we seem to proceed with entire safety in asserting, that they must have been constructed by tribes, or nations, who were *extremely numerous.*

The Rev. Mr. Harris, of Massachusetts, has lately favoured the public with some additional observations concerning the design of these works, and concerning the people by whom they were erected.‡ But this gentleman's hypothesis on the latter subject is not, in any essential respect, different from

* From Barton's Philadelphia Medical and Physical Journal. Vol. ii. 1805.

† A Letter on the supposed Fortifications of the Western country, from Bishop Madison, of Virginia, to Dr. Barton. See Transactions of the American Philosophical Society. Vol. vi. Part 1. No. 26.

‡ Journal of a Tour into the Territory North-West of the Alleghaney Mountains, made in the Spring of the year 1803, &c. &c. Boston: 1805.

that

Ancient works in America.

that which the editor of this Journal published, several years ago, viz. first in 1787, and again in 1796.*

Some time ago, I was in conversation with a Mr. Nathaniel Brittain, living in Mount-Bethel township, on the topic of some of our American antiquities. He told me, that a Mr. Gaston, and another person, whose name I have forgotten, who were formerly his neighbours, had emigrated to some of the western counties of this state (Pennsylvania), and a few years since paid him a visit, when he was informed by them, that, at some salt-lick, which afforded a small quantity of brackish water (I think he said on Gaston's land), under a belief, if they were to dig a hole to some depth into the earth, a greater quantity of salt-water might be acquired, they, accordingly, dug down some depth, when they came to the side of a rock, from whence the water seemed to filter; that on clearing the earth from the rock, they found an old pot (I forgot wether of iron or earth), a shovel, and some tubes, through which the water appeared to have been conveyed.

At another place, at some flat near a river, a man began to dig a well, and after working to some depth, he came to a large flat stone. This he worked out, and found it to cover an old walled well, with water at the bottom.

I should think these to be subjects worthy of the inquiry of your friend B*****, and, if the reports were found to be true, they would make a curious addition to his work on American Antiquites.

Mr. JOHN ARNDT.

Letter to Mr. John Heckewelder, dated Easton (Pennsylvania), March 16th, 1800.

* See Observations on certain parts of Natural History, &c. &c. London: 1787;—and Papers relative to certain American Antiquities, &c. &c. Philadelphia: 1796. 4to.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1806.

ARTICLE I.

Outline of the principal Inventions by which Timekeepers have been brought to their present Degree of Perfection. Received from a Correspondent.

The rewards lately given by the commissioners of Longitude; render the history of chronometrical inventions interesting;—more especially on account of justice.

TWO considerable rewards having been lately granted by the Board of Longitude for improvements in timekeepers,* it becomes interesting to science to point out the progress which the art of chronometry has made for some years, and to establish to the real authors the property of their respective inventions, as far as evidence can be collected from books or credible reports: And this is the more important to truth, because the account of those rewards has been publicly stated in terms so positive, and with pretensions so high, that those unacquainted with the true history might be induced to think that something attempted in vain before, had been actually performed by those claimants; and that machines had

* To Mr. Arnold for his father's improvements, and to Mr. Earnshaw for his own. See Nicholson's Journal, vol. xiii.

now for the first time been produced, capable of determining the longitude at sea.

General description of machines for measuring time.

The machines which, for centuries, have been commonly used to measure time, consist of a movement, or train of wheels, drawn by a weight or spring, and a regulator, the object of which is to keep the motion of the train within the required degree of uniformity. A continual rotatory motion, which constantly tends to accelerate, is thus corrected by means of an alternate motion; while the power which carries round the movement restores, also, to the regulator the action lost by friction and other causes. The mechanism, by which the two principal parts act on one another, is called the escapement; and this most admirable contrivance may be reckoned the distinguishing characteristic of the modern art of timepiece making. It is not the object of the present inquiry to trace the history of the successive alterations and improvements made in the construction of clocks and watches upon that principle; but briefly to mention such inventions as have been proposed from the middle of the seventeenth century, and have had a direct influence on the progress of chronometry.

The train and the regulating part.

The escapement is the great character of modern clocks.

Great improvement *of the train by the remontoir;*

A very ingenious invention to improve the movement, or that part of the timepiece which is corrected by the regulator, is the *remontoir*. The action of the movement on the regulator suffers continual alterations, by the inequalities which proceed from the nature of the weight or spring, and by those which occur in the friction of the pivots and teeth of the wheels. In order to prevent those alterations from affecting the regularity of the going of the machine, the usual weight or spring has been employed, only to wind up, at very short intervals of time, a secondary power, which may thus be supposed to be uniform, and is the one which acts immediately on the regulator, by means of the escapement. This contrivance seems to have been executed about 1600;* but Christian Huygens is the first † to whom we are indebted for an explanation of this kind of mechanism, with a weight; and he probably conceived that idea without any previous hints from others. Leibnitz, however, a little afterwards ‡ published the

—which is a contrivance for winding up very frequently that power which acts on the regulator: the action is thus more uniform.

Invented 1660. —explained by Huygens, also by Leibnitz:

* Histoire de la Mesure du Temps, par F. Berthoud, vol. ii. p. 41.

† Horologium Oscillatorium, 1673, p. 18.

‡ Philosophical Transactions, 1675, No. 113.

inven-

invention of a spring remontoir, as a new thing; and Dr. Hooke on this occasion* asserted, that he had also known that way ever since A. 1660; but, as he had never declared it to anybody, he could not say that it had been taken from him. The principle of the remontoir has since been adopted by J. Harrison and some others, but it has never become general; and it is supposed that we actually possess more simple means of attaining the same advantages.

—claimed by Hooke:

—and adopted by Harrison.

The greatest step ever made in the improvement of the regulator of timepieces was the application of the pendulum for that purpose; and the merit of the invention cannot be denied to Huygens, who executed and explained it in the most satisfactory manner in his *Horologium Oscillatorium*, though certainly he was not the first who conceived the idea of employing the pendulum to measure time, nor even perhaps the first who thought of attaching it to a clock. The complete practical benefit of the pendulum was not, however, the result of the profound investigations of that great philosopher to procure cycloidal vibrations; and its accomplishment is due to the invention of the anchor and dead-beat escapement, which, permitting only narrow vibrations, obviated the inconveniences observed when the pendulum, suspended on a thread with cheeks to modify the vibrations, was used with the old recoil escapement. Huygens also invented the application of a pendulum with conical or circular motion, and the theory and contrivance, used by him for that purpose, do great honour to his genius; but the success did not answer his expectation, and it does not appear that any attempt has been since made to render those principles useful in practice.

Great improvement *of the regulator* by the application of the *pendulum*:

—ascribed chiefly to Huygens.

Cycloidal vibrations.

Anchor escapement.

Conical pendulum.

Huygens constructed a timekeeper for finding the longitude at sea, which is the first practical attempt of that kind that was attended with any degree of success; though the notion of determining the longitude by that method was proposed so early as the beginning of the seventeenth century by Gemma Frisius,† and followed by Metius and others. A timekeeper of the construction of Huygens was tried by Major Holmes in 1664,

The first timekeeper for sea, by Huygens, with a pendulum.

* Philosophical Transactions, 1675, No. 118.

† De Principiis Astronomiæ et Cosmographiæ, 1530.

who gave a favourable account of it;* and some other trials were afterwards made in France and Holland with various success, which the author † attributes to the bad management of those who had charge of the machines, but which we may venture to ascribe principally to the nature of their construction. Huygens' timekeeper was maintained by a spring, regulated by a pendulum, and the whole was suspended in such a manner as was supposed most proper to procure the indispensable stability.

Timepieces with a pendulum are best for land use:

Timepieces with a pendulum regulator are certainly the most perfect, when they are kept in a fixed situation; and, for that reason, these are the only sort used in astronomical observatories. But external motion is so contrary to the regularity of their performance, that no sea chronometer has been since attempted to be constructed upon that principle. The balance regulator remained, as affording the only method by which the desired uniformity might be obtained in portable machines; and the great improvement made in that regulator, by the addition of a spiral spring, may be considered as one principal cause of the perfection which has been since attained in them. The first invention of attaching a spring, to give to the balance by its elasticity a power which renders the action of this sort of regulator similar to that of gravity in the pendulum, is undoubtedly due to Dr. Hooke, though it is not so clear whether he ever applied it in the shape of a spiral, as has been so long practised since. F. Berthoud, in his *Histoire de la Mesure du Temps*, (vol. i. pp. 134 to 141), gives a body of extracts from several works relative to this subject; and concludes, that Dr. Hooke only applied a straight spring to the balance, and that M. Huygens improved upon that idea, and contrived the spiral spring, which is more favourable to the vibrations of the balance. M. Huygens, indeed, applied in France a balance spring, the account of which has been published in the Philosophical Transactions for 1675, No. 112; but Dr. Hooke, in the *Postscript* to his Description of Helioscopes,‡ asserts that the hint was taken from the experiments he had made in 1664,

—those with a balance for sea.

The balance spring was invented by Robt. Hooke.

Historical remarks, &c. on the balance spring.

* Philosophical Transactions, 1665, vol. i. p. 13.

† Horologium Oscillatorium, 1673, p. 17.

‡ Lectiones Cutlerianæ, 1679.

in

in Gresham College, where *he explained above twenty several ways by which springs might be applied to do the same thing;* and complains of Mr. Oldenbourg, secretary to the Royal Society, for his conduct and supposed partiality to the Dutch philosopher. Mr. Oldenbourg justified himself against that accusation immediately; but it is worthy of remark, that in his account of the matter laid before the public,* while he mentions that Dr. Hooke's application of the spring to the balance had failed repeatedly, and gives several reasons to shew the fairness of his proceedings, no difference of figure is stated by him to distinguish the manner of applying that principle by the two competitors; and, as this difference would have proved a very strong ground of exculpation to Mr. Oldenbourg, his silence upon the subject affords reason to suppose that it did not exist, and that Dr. Hooke had, before Huygens, actually applied, or shewn the method of applying, the balance spring in the shape of a spiral.†

Advantages of jewelling the working parts of timepieces.

In relating the progress of timepiece making, we must not omit mentioning the use of precious stones, particularly rubies, to form the holes in which the pivots of the wheels turn, and the pallets upon which the action of the teeth is exercised. These jewels, by the high polish given to them, reduce the quantity of friction; and, not being liable to the wear which takes place in metal rubbing upon metal, the machine with that addition, not only becomes more durable, but acquires a degree of uniformity in the motion of the pieces, which is very favourable to the regularity of its going.

The inventor of jewelling not well known: it is ascribed to Fatio.

It does not seem easy to discover, with sufficient certainty, the date and author of the application of jewelling to clocks and watches. F. Berthoud says ‡ that the art of perforating rubies

* Philosophical Transactions for 1675, No. 118.

† It is asserted (Supplement to the Encyclopædia Britannica, article watchwork, vol. ii p. 785) that Dr. Hooke first applied the straight spring, and then the cylindrical or helical spring, such as has been employed by the late Mr. Arnold, which he afterwards gave up for the flat spiral spring; but I have not been able to discover the proof of this statement.

‡ Supplement au Traité des Horloges Marines. Introduction, p. viii. note.

(percer

(percer des rubis) was proposed in France, during the regency, by M. Fatio, a Genevese, who, not meeting with encouragement there, came to England, where his secret was received and adopted; and he refers for his authority to H. Sully's *Regle Artificielle du Temps;* but I have not found this account in the edition of this book dated 1717, at Paris; which is the only one to which I have access: the passsage alluded to may probably appear in the edition of 1726, published after the death of the author, by Julien le Roy. Be this as it will, the art of jewelling, of which Harrison availed himself with judgment, has been ever since, and continues to be, a material article in the construction of timekeepers.

—and was since used by Harrison, &c.

Henry Sully laboured in making timepieces for sea.

The above-mentioned Henry Sully, an Englishman, who settled in France in the beginning of the last century, may be esteemed the first of those great artists who have carried the manufactory of watches to the high degree of perfection, which it still maintains in that country. He laboured, with uncommon skill and perseverance, in making timekeepers for sea on a new plan, of which he made a trial at Bordeaux in 1726; but, the author dying immediately afterwards, his construction remained useless, and has never been copied or improved since that time. Indeed the method, according to which he intended to effect the isochronism, by means of a lever suspended on a thread, or flexible wire passing over curved cheeks, to modify the vibrations of a vertical balance, though not the same in principle, is so similar in its inconveniencies to Huygens' pendulum, that we cannot wonder if it has failed before, nor expect that it will ever be useful for portable machines in future.*

His regulating part has never since been used, and is exceptionable.

Sully made many improvements, and in particular he introduced friction rollers.

Sully made and published in his works a variety of ideas and inquiries; but the principal practical improvement which he seems to have at first proposed, and has since his time been frequently employed in timepieces, particularly in France, is the application of rollers to diminish the friction of pivots in timekeepers.

John Harrison obtained the reward of the

Before Sully's death, John Harrison had probably made some progress in his labours for the improvement of time-

* Sully published an explanation of his timekeeper, in a book intituled, Description abregée d' une Horloge de nouvelle Invention, pour la juste mesure du Temps en Mer. 1726.

pieces,

pieces. That extraordinary man, having produced the first machines, which, upon repeated trials, met with success, to the extent required for the great reward offered by parliament, must be reckoned the father of modern chronometry; and his long and active career has proved extremely useful by stimulating with so bright an example other artists to similar endeavours. The principles of Mr. Harrison's watches are well known; and, as most parts of his construction have been superseded by more simple contrivances, we shall only mention the principal inventions of which he appears to be the author, and which are still used by the watchmakers of the present day.

British parliament for his timepiece.

The going fusee is one, among those inventions,* which has proved the most generally useful in practice. By this simple mechanism, the main spring, while the watch is going, acts on an intermediate short spring, which Harrison calls the secondary spring, and is constantly kept bent to a certain tension by the former; and, when the watch is winding up, and the principal spring ceases to act, the secondary spring being placed in a ratchet wheel, which is hindered from retrograding by a click, continues the motion without alteration. Other contrivances have been proposed, and executed, to make timepieces go while winding up; but none which, like this, combines the advantage of simplicity, and the property of providing a supplementary power, which is equal to that of the main spring when its action ceases. And it is to be observed, that the utility of the going fusee, which has induced manufacturers to introduce it into all good watches, is peculiarly important in those timepieces which have not the power of setting themselves in motion, as is the case with the best modern escapements.

Harrison invented the *going fusee.*

Description of this mechanism:

—its advantages:

—and great utility in chronometers.

Harrison invented also a compensation for the effects of heat and cold, which at the time was perfectly new, and has led to the improvements made afterwards in that essential requisite of timekeepers.

Harrison's compensation for the effects of heat and cold.

* We have heard that this piece of mechanism was first invented by a maker of kitchen jacks; and, if so, it is not impossible but that Harrison might have benefited by this contrivance, before his application of it to timekeepers.

The

The balance and its spring are more affected by temperature than the pendulum.

The alterations to which the length of the pendulum is liable by the different degrees of heat and cold, affect the going of clocks with that sort of regulator; and watches, with a balance, are still more subject to irregularity from that source; because not only the balance expands or contracts, according to the rise or fall of the thermometer, but the regulating spring itself, while it suffers similar changes, becomes weaker or stronger; so that, from these causes, a timepiece must go slower or faster in too great a proportion to be overlooked or neglected.

Graham first compensated this in the pendulum by two metals.

Graham* is the first who thought of applying two metals of different expansibility, to correct the errors proceeding from the variation of temperature in a pendulum; but as he seemed to have had in view to effect it immediately, without the aid of mechanism, he was obliged to fix on steel and mercury, these being the metals which offered to him the greatest difference of dilatation and contraction.

Harrison's gridiron pendulum.

Harrison, by multiplying the bars, increased the total length of the two metals acting on one another, without exceeding the limits of the pendulum; and thereby produced a sufficient compensation with brass and steel in the compound, or gridiron pendulum, which has been almost universally adopted ever since.

His expansion curb for a balance spring:

—composed of two plates of different metals rivetted together.

This contrivance could not be easily applied to balances; but Harrison, following still the principle of the different expansibility of metals, applied it in a manner which had not been thought of before, and made it act on the spiral spring, in order to produce the desired compensation in the regulator. This method is described as follows:* " The thermometer kirb is composed of two " thin plates of brass and steel rivetted together in several " places, which, by the greater expansion of brass than steel " by heat, and contraction by cold, becomes convex on the " brass side in hot weather, and convex on the steel side in " cold weather; whence, one end being fixed, the other end " obtains a motion corresponding with the changes of heat " and cold, and the two pins at the end, between which the " balance spring passes, and which it touches alternately as " the spring bends and unbends itself, will shorten or lengthen " the spring, as the change of heat and cold would otherwise

* Philosophical Transactions, 1726.

* Principles of Mr. Harrison's Timekeeper, p. xii. notes.

" require

" require to be done by the hand in the manner used for regu-
" lating a common watch."

This kind of compensation has been since applied in other ways; but the method, in general, is liable to some material objections, on account of its altering the length of the balance spring, and the difficulty, perhaps impossibility, of effecting with it an accurate adjustment. Mr. Harrison himself was aware of these objections, and expressed the well known observation,* *that if the provision for heat and cold could properly be in the balance itself, - - - the watch- - - would then perform to a few seconds in a year.* By the watch, Mr. Harrison meant his own *longitude timekeeper;* and we have now sufficient reason to believe, that he overrated its merits, though the construction had been improved with the desired invention, upon which he set so great a value; but that assertion, which has been repeated † and strengthened by men of learning and good judges of mechanics, shews, at least, the importance of the desideratum, which seemed to be still wanting to complete the perfection of chronometry.

Objections to compensation on the spring.

Harrison proposed to put the compensation in the balance itself, but did not contrive any mean of effecting it.

Pierre le Roy, eldest son and successor to Julien le Roy, the companion and friend of Henry Sully, had the merit of accomplishing that great desideratum. In the chronometer, which was presented to the king of France the 5th August 1766, and obtained the prize of the Academy of Sciences of Paris the 31st of the same month, that author executed a compensation in the balance, which he has fully explained in his description of that machine.‡ This compensation is composed (Fig. 1, Pl. VII.) of two thermometers, *t* K *t* A K, of mercury and spirits of wine, made each in the form of a parallelogram, except in the upper branch, which bears the

Peter le Roy completely effected this purpose at Paris, in 1766.

—by mercurial thermometers in the balance of a chronometer, which obtained the academical prize of that year.

* A Description concerning such Mechanism as will afford a nice, or true Mensuration of time, &c. By James Harrison, 1775, p. 103.

† See Mr. Ludlam's letter to Dr. Maskelyne, in the Report from the committee to which Mr. Mudge's petition was referred, pp. 96 and 97.

‡ Mémoire sur la meilleure Maniére de mesurer le Temps en Mer, qui a remporté le Prix double au Jugement de l'Academie Royale des Sciences. Contenant la Description de la Montre à Longitudes, presentée à sa Majesté le 5 Août, 1766. Par M. le Roy, Horloger du Roi. pp. 41 to 44. This Memoir accompanies the account of Cassini's voyage in 1768, published in 1770.

ball containing the spirits of wine, and is a little bent downwards; the mercury is in the lower part, and the vertical branch of the tube, A K, is open at the upper end. These two thermometers are placed opposite one another, the axis of the balance being in the same plane with the centrical lines of the tubes; and the thermometers and balance are solidly attached together, and form a well poised and steady regulator. At the middle temperature of the atmosphere, the quicksilver stands at K A *t* K; but, when an increase of heat occurs, the alcohol, by its expansion, forces the mercury from the exterior branch, *t* K *t*, towards A K, and a portion of the mass of the regulator contracts by approaching the centre. On the contrary, if the variation consists of an additional degree of cold, the mercury moves towards the exterior branch, and the weight towards the circumference of the balance becomes greater. Thus, if the thermometers are well adjusted, the effects of all the changes of temperature in the balance will be compensated, and the regulator will act with the same uniformity as if its figure were not liable to such alterations.

Peter le Roy did not know of Harrison's expansion curb when he made his thermometrical balance:

P. le Roy invented and executed this method of compensation before he became acquainted with Harrison's contrivance of the compound metallic thermometer; and he avows,*

* Mémoire sur la meilleure Manière de mesurer le Temps en Mer, &c. p. 56 and 57.—This being an interesting point, I shall subjoin a literal copy of the passage of P. le Roy's memoir, in which that author states his manner of making the compensation balance.

——quand j' ai cherché à compenser l'effect du chaud et du froid par des lames de cuivre et d' acier, rivées ensemble, comme M. Harrison, j' ai tenté, non de changer la longueur du spiral, mais de faire approcher ou éloigner par ce moyen, du centre du balancier, une partie considérable de sa circonférance. Pour cela, j' ai employé un balancier (fig. 2) composé de deux demi-cercles formés chacun d' une lame de cuivre et d' une d' acier, réunes comme dans le thermomètre de M. Harrison.

L' effect répondoit assez à mes intentions, j' ai même observé au moyen de l' index *i*, conduit par des semblables lames *ll* (fig.3), que par le froid et le chaud, le mouvement de ces lames suivoit assez exactement la marche du thermomètre : il en résultoit une compensation de la chaleur et du froid, dont ou pouvoit augmenter ou diminuer l' effect à volonté, en mettant plus ou moins de masse aux extremités des ces demi-cercles.——

that

that, if he had been in possession of it before he began this part of his labours, he would probably have made use of the same principle, but applying it in a very different manner, *videlicet*, to enlarge by cold, and contract by heat, the circumference of the balance, preserving the spring untouched. He proceeds to state how a balance may be constructed with compound metallic pieces, to effect the compensation, and gives a figure to elucidate his plan, of which Fig. 2, Pl. VII. is a copy; adding that, according to this method, the balance may be easily adjusted by means of small balls, or weights, which are to be attached to the ends of the metallic curves. The curves, being made of two plates of different metals, with the one most affected by variations of temperature at the outside, it is clear, that heat will move the balls placed at the extremities towards the centre, and that cold will move them in the contrary direction; producing, by this contraction or expansion, the same sort of compensation as that of the mercurial thermometer explained before. P. le Roy did not remain satisfied with the simple suggestion of this contrivance, but actually put it in practice; and employing a register, such as is represented in Fig. 3, ascertained by experiment that the mechanism performed well, and corresponded pretty exactly with the other thermometers. After all these investigations, he concludes, by giving the preference to his own mercurial thermometer, because he thinks it more accurate and steady, as well as more fit to secure, in all temperatures, an uniformity of weight to the whole circumference of the regulator, than the compound metallic balance; and, under those points of comparison, he may be right in his choice; but certainly the last thermometer seems better adapted for small and portable machines, and has answered, during repeated trials, so well, that we must believe it fully entitled to the favour which it has obtained in practice.

—but he give a drawing of a balance of his invention on the principle of that curb; the arms or arcs of which act by flexure and are adjusted by moveable weights.

He proved the effect of such arms by experiment;

—but gave the preference to mercurial compensation.

The compensations in the balance, applied at present to the best chronometers, are essentially the same as that so well explained and published so long ago by P. le Roy; but, in this country, the invention has been generally ascribed to the late Mr. Arnold, who, in 1782, took a patent for it; and a degree of merit has been attributed to him on that account, proportioned to the supposed difficulty of the desideratum expressed by

Peter le Roy's invention is the same as is now used: but though so publicly declared in the face of the French government and academy in 1766, Arnold took an

English patent for it in 1782.

by Mr. Harrison. It would be hard, indeed, to find a similar instance of an invention, the first author of which may be so clearly ascertained, and from which a second inventor, if Arnold can be allowed as such, has derived so much credit.

Concerning the escapement.

We have not yet taken any notice of the improvements made in the escapement; because, after all the plans proposed for this most essential part of chronometers, the principle of what is called the detached escapement, is the only one now used; and, being established upon long experience, seems to merit the preference given to it over all the constructions proposed till now. We shall content ourselves with stating in a general manner the beginning and progress of that escapement.

Explanation of the bad effects of the maintaining power when connected constantly with the regulator.

In all the escapements known till the middle of the last century, the escape wheel was in continual contact with the pallets belonging to the axis of the balance wheel; and the friction, proceeding from this circumstance, may be considered as a principal source of irregularity in the going of the watches. Suppose, that a regulator should be made so perfect as to be exactly isochronal, while vibrating in a free situation: that advantage would be diminished or lost as soon as it was placed in connection with a train of wheels; and the errors would be more or less, according to the nature and quantity of friction in the escapement. It would be, therefore, extremely useful to secure to the regulator a perfect liberty of vibration, except during the short intervals of time which may be necessary for the action of the escape wheel, to give it a new impulse. This ingenious idea was also started by P. le Roy, and carried into execution by the same artist, in a model which he presented in 1748 to the Academy of Sciences of Paris, and is described in the collection of machines approved by that society for the same year.*

A balance, tho' itself perfect, would not measure time if so connected.

Peter le Roy also contrived the first detached escapement in 1748.

Description of Le Roy's escapement.

That escapement is represented in Fig. 4, Pl.VII.; G H is the escape wheel, the profile of which is shewn at *gh*, and TV the balance. The curved pallet, AE, is affixed to the axis under the balance; and on the same axis, but above the balance, and under the spiral spring, is attached the half-cylinder, C I, the end, C, of which is round, and placed in such a manner, that

* Vol. vii. No. 481, p. 385, intituled, Echappement à détente, invente par M. le Roy, Fils ainé, Horloger.

a line

a line drawn from that point to the centre of the balance would form with the curve, A E, a mixed angle of about 80°. QPG is an angular lever, turning on pivots at P, the branch of which, PX, when in contact with one of the teeth of the escape wheel, stops its motion; and the spring, R M, tends to keep it disengaged against the pin K. The effect of this construction is as follows:

The effect or action of Le Roy's escapement.

Suppose the action of the tooth, D, of the escape wheel to give tension to the spiral spring, and make the balance move round an arc more or less extensive; the end, C, of the half-cylinder or roller, at the same time, pushes the angular lever, Q P X, by its end, Q, making the end of the branch, P Q, rest upon the circumference of I C; and when the tooth, D, quits the pallet, A E, the tooth, G, comes in contact with the end, X, which then continues the motion of the angular piece, Q M X, till the tooth and the end, X, are completely engaged and remain at rest. In this situation, no part of the branch, P Q, touches the round part, C I; and the balance proceeds in its vibration as if it were insulated. Its velocity being soon destroyed, by giving tension to the spiral spring, the reaction of this spring brings it back with an accelerated motion, and at the second vibration the pallet, A E, comes into contact with the escape wheel, and by its action on the tooth next following D, causes the wheel to recoil, by a space, which may be equal to half the distance between two teeth. The branch, X, of the lever, in consequence of that motion, becomes disengaged out of the teeth of the wheel, and, by the action of the spring, R M, falls against the pin, K; after which, the escape wheel gives a new impulse to the balance, pushing the pallet, D E, in the contrary direction; and the vibrations proceed alternately, in the manner explained before.

An escape wheel is kept in repose by a lever detent. The balance unlocks the detent and receives an impulse or stroke on a pallet thro' a part of every second vibration; and during great part of its course it is free and detached.

It has been asserted, but not proved, that Dutertre invented a detached escapement earlier than Le Roy.

While we give this escapement as the first of that kind ever invented, it is proper to remark, that according to report,* Jean Baptiste Dutertre, the elder, a very skilful watchmaker at Paris, in the beginning of the last century, had thought of, or actually contrived, a detached escapement; but as his inven-

* Traité des Horloges Marines, par F. Berthoud, 1773, p. 97. Etrennes Chronométriques pour l'année, 1759, par P. le Roy. p. 88.

tion,

tion, if true, was never published, nor even mentioned, till after other persons had produced their labours on the subject, we must ascribe to P. le Roy the original idea, as well as the first execution, of that ingenious construction; and in this opinion we are strengthened, by observing that the model of 1748 was received by the Academy of Sciences, both as new and advantageous; and that some years after, when a new detent escapement, by M. Platier, was submitted to the same society, the commissaries, M. Montigni, and M. Vaucanson, who examined it, and were certainly the most competent judges in such a matter, expressly declare in their report that M. le Roy was the first who ever thought of this sort of escapement.*

Description of another improved escapement by Peter le Roy.

P. le Roy contrived also another detached escapement, which is an improvement upon the former, but according to the same principle; and he applied it to the timekeeper which was presented to the Academy of Sciences in 1766, and afterwards tried at sea by order of the French government. In this construction, the escape wheel (Fig. 5, 6, and 7, Pl.VIII.) is made with teeth which are very light and at a considerable distance from one another, it being meant that their power should proceed from the length of the lever; and they act on the balance by means of a pallet, *p*, adapted to the circumference of the latter. The action of the escape wheel, except the time which is requisite to restore to the balance the power lost, once in every two vibrations, is suspended by a compound detent, D *c* H *c* F, (Fig. 6, 7, and 9), very different from the mechanism employed in the former escapement.

Its effect or action described.

The escape wheel being stopped by the detent at D (Fig. 6), the balance vibrates first from A to *i*, and afterwards from *i* to A. On this return, the balance, by means of a pin, placed on its upper face at *i*, pushes the arm or lever, F H; and then the arm, D H, gets out of the circumference of the wheel, and the arm, *e* H, coming into action, stops the following radius, K *r*, of the wheel which falls upon it. This disposition of the respective pieces, which Le Roy calls the *preparation*, is represented in Fig. 7. In the following vibration, the escape wheel restores to the balance its lost power, by means of the pallet, *p*, in this manner:

* Observations sur la Physique, par M. l'Abbé Rozier, t. iii. part. i. Juin, 1774.

ner: A pin, which is placed as the preceding one, but in the under face or plane of the balance, pushing the arm C H, gets the arm, *e* H, out of the circumference of the wheel, and introduces D H; so that when the pallet, *p*, arrives at F, the wheel being free, the radius, F *r*, gives a new impulse to the balance, and impels the pallet, *p*, till it is stopped by the arm, D, of the detent, as in Fig. 6.

To prevent the detent from being displaced by the effect of external motion, a circular curb, *i* A, *i* A (Fig. 5, 6, and 7), has been adapted to the circumference of the balance, near each pin, which disengages the detent: but, the arms of the detent can only touch the corresponding curbs, in consequence of the most violent shakes.

The improved escapement of Le Roy's differs from his first.

1st.—It has less connection and no recoil in unlocking the detent.

The construction of this escapement principally differs from that of 1748 in these three points: 1st.—In the last escapement, the second vibration, or return, is permitted to be completed, and it is not till the balance comes again to move in the first direction that a new impulse is given to it, in the middle of the whole arc of vibration; while, in the former, the free return of the balance proceeds no farther than the place where it received the first impulse, where a new action is opposed to it. 2d.—In the second escapement, the pallet is situated near the outer circumference of the balance, with a view to render the impulse upon it more favourable to circular motion, without a consequent action on the pivots; while in the former escapement the pallet or edge of the half-cylinder is near the centre. 3d.—The mechanism of the detent in the new escapement, having no springs, is also different from that of the other, which depended upon that description of power. P. le Roy was led to contrive the new detent, because he wished to avoid the inconveniences arising from the use of the springs for that purpose, inconveniences which are considerable in his opinion, on account of the loss of power which takes place if the springs are strong, and of the uncertainty of their performance if they are weak.

2d.—The action affects the balance pivots less, because it is given at the circumference.

3d.—The detent has no spring.

All the modern escapements, which have been much used, are essentially the same as those of Le Roy's.

From the two preceding escapements of P. le Roy, are derived, without material improvements, (unless the spring detent or locking spring should be esteemed one), all the detached escapements which have been executed, to any considerable number, from that to the present time.

About

Mention of the detached escapement of Mudge; contrived in 1755, and different from that of Le Roy.

About the year 1755, according to Count de Bruhl, the late Mr. Thomas Mudge invented a detached escapement, and applied it to a watch which he made for the king of Spain, Ferdinand VI. This is the same escapement that was used by the late Josiah Emery in his chronometers, some of which have gone very well. It differs from the constructions which we have already explained, both in the detent and in the communication of the impulse, which in this mechanism takes place at every vibration; but the date will not suffer us to consider it as the first invention of the detached escapement.

The latter escapement of Mudge is not, properly speaking, detached, but it winds up springs, connected with the balance, every vibration.

Our design will not lead us to be more particular respecting the invention of another escapement applied by Mr. Mudge to the chronometers, for which he was rewarded by parliament. We shall merely observe, that the principle of that escapement is not free, and it could only be ranked among the detached escapements, in consequence of giving to that appellation a different sense from the usual meaning attached to the term. The peculiar mechanism of that machine consists in a kind of double remontoir, which is placed within the escapement, or beyond the whole of the train, and not antecedent to the escape wheel, as in the remontoirs of Huygens and Leibnitz; consequently the maintaining power of the timekeeper, through the train of wheels, acts only during a short portion of the vibration; but then the remontoir, or secondary power, which is composed of two springs, by means of their alternate winding and unlocking, is almost in constant action upon the balance. The author himself declared,* that this escapement could not, with propriety, be called detached; and it is rather surprizing, that F. Berthoud should,† notwithstanding, have placed it in that class, in the account he has given of it. His opinion of this construction seems, however, well founded; and we agree with him in thinking, that it is too complex, and requires too nice an execution, ever to become generally useful.

* Letters of Mr. Mudge, attached to the Description of his Timekeeper, 1799, p. 152. This escapement is also described in our Journal, quarto series, vol. ii.; and in the Phil. Trans. for 1794.

† Histoire de la Mesure du Temps. vol. ii. p. 44.

In

Berthoud ascribes the invention of the detached escapement to P. le Roy, Mudge, and *himself*.

In the *Histoire de la Mesure du Temps*,* the invention of the detached escapement is ascribed to three different persons, who accomplished it separately, and upon distinct principles, without any communication with one another; P. le Roy, T. Mudge, and the author, F. Berthoud, himself. We have already noticed the labours of the two former, and it remains for us to state what right the last may appear to have to the honour he claims. The title of P. le Roy to priority cannot be invalidated, and what F. Berthoud has written upon the subject is of so much later date, and his ideas seem so closely derived from those of the preceding author, that it would not be fair to grant him the share he assumes in point of originality on this occasion. Berthoud published his *Essai sur l'Horlogerie* in 1763; but not one word is to be found in it, respecting the principle of the detached escapement, though P. le Roy had made it publicly known fifteen years before. This silence or omission in a treatise of two volumes in quarto, respecting a construction so remarkable, appears to be inconsistent with the subsequent pretensions of the author; and this circumstance, at least, must prevent our supposing that he had applied with much attention to the subject. It was not till 1773 that Berthoud took notice of the detached escapement, in his *Traité des Horloges Marines*, where he describes several constructions of that kind, giving them as the result of his own inquiries, upon which he says he had been engaged, ever since the beginning of his labours upon timekeepers for sea. In that work, Berthoud also states,† that in 1754 he made a model of a detached escapement, which he shewed to M. Camus, to be presented to the Academy of Sciences, when that learned gentleman told him, that M. Dutertre, the father, had had the same idea before.

Reasons, shewing that Berthoud has no claim to the invention.

He took no notice of any detached escapement till 1773.

Berthoud avoided mentioning Peter le Roy.

It might have been of service to him, to have been at the same time reminded of what P. le Roy had done in the same way; for the name of that artist is, in the whole, so studiously avoided, as to raise a suspicion of want of candour in the writer, who, on several occasions, was the declared rival of that great mechanic. It may also be remarked, that F. Berthoud was, even at that time, so far from judging properly of the detached escapement, that he concludes his book on marine time-

* pp. 24 and 25.

† p. 97, note b.

—and even then preferred the dead-beat with ruby pallets to the free escapement.

keepers * with an article entitled, *De la préférence que l'on doit donner à l'échappement à repos à palettes de rubis, sur celui à vibrations libres ; constatée par des éxperiences décisives ;* while Le Roy, from the conviction of the accuracy of his construction, ventured to assert (in his *Précis des differentes Recherches,* &c. p. 37), that chronometers would, in future, be made, according to those principles, without material variations. Long experience has already justified M. Le Roy's ideas, and established, beyond doubt, the great advantages of the detached escapement; and F. Berthoud, in his history, published in 1802, has, at last, done something like justice to that great watchmaker, and his construction; but, at the same time, has associated himself to the honour of the invention of the detached escapement, in terms, which, so far from appearing well established, are rather in contradiction to the evidence afforded by an attentive comparison of his preceding works.

Peter le Roy asserted the superiority of the latter; and Berthoud, in 1802, admits his claim, but demands to share it.

Of the different constructions of the detached escapement published by F. Berthoud, which indeed differ from one another merely in the contrivance of the detent, and are all made to act with springs, we shall only notice the kind which has been the most generally used both in Great Britain and France. Fig. 10, Plate IX. represents this escapement, as copied from the *Traité des Horloges Marines*, 1773, Fig. 5, Plate XIX. The escape wheel, A, is stopped by the arm, B *d*, of the detent, *f* B *d*, while the balance vibrates in two directions: the detent moves on pivots, and is pressed by the spring, *a*. C is a circle, or wheel, attached to the axis of the balance, but of smaller dimensions, and has a pallet, *c*; which, when it turns from *c* towards *e*, acts on the arm, *f*, and disengages the escape wheel. At the same moment the pallet, *g*, which is placed within the thickness of the circle, C, and stands as high as the escape wheel, receives an impulse from the tooth, *i*. During this action, the pallet, *c*, quits the arm, *f*; and the detent, pressed by its spring, drops into the escape wheel, to meet the succeeding tooth, and keeps it at rest, after the communication of power is completed. The tooth, *i*, is, at this time of stopping, disengaged out of the pallet; and the balance, being free, finishes its vibration. When the balance returns in the direction from *e* to C, the pallet, *c*, acts on the back of the arm, *f*; but this part is flexible, and forms an in-

The construction of the detached escapement of Berthoud, which has been most frequently used. Description and drawing. The detent has a back spring, and there is no recoil.

* Traité des Horloges Marines, p. 576.

clined

clined plane, over which the pin slides with little resistance, and without disturbing the detent. At the next vibration, the detent is disengaged, a new impulse is communicated as before; and the actions, already explained, continue to be performed in succession.

F. Berthoud thinks that construction the simplest and safest in practice, and gives it again as such in his later works,* with a little alteration in the arrangement of the pieces, as represented in Fig. 11, which is copied from Fig. 9, Plate XIII. of the *Histoire de la Mesure du Temps*.† In this construction, Berthoud attached a very delicate spring to the outside of the arm, *n*; which, projecting a little beyond the extremity, serves for the purpose of yielding in one vibration, and unlocking the detent in the next, instead of rendering the arm itself flexible. The additional arm, *k*, is only intended to stop the wheel, when the balance is taken out; and the other parts of the figure, after what has been said in the preceding paragraph, need no farther explanation.

The same escapement a little varied by Berthoud. It has the back spring and an unlocking spring.

In the construction of the detached escapement adopted by Berthoud, the impulse of the escape wheel is communicated to the balance, not on the circumference of the balance, as in Le Roy's second escapement, but on a circle, or pallet, situated considerably nearer the axis, as in the former escapement. The detent also acts by means of springs, as in Le Roy's first plan, and not by the sort of mechanism which that author thought preferable. On these two points, the practice of succeeding watchmakers has continued in conformity with those two retrograde steps; but, whether on account of real advantages, or merely from the greater facility of execution, need not, on the present occasion, be discussed.

Observation that Berthoud has not given his impulse to the balance rim, nor rejected springs, as Le Roy did.

We come now to the constructions used at present, by the English watchmakers; and shall begin with that of the late Mr. Arnold, as described in his statement, presented by his sons to the Board of Longitude.

English constructions of free escapements.

* De la Mesure du Temps, ou Supplément au Traité de Horloges Marines, &c. 1787, chap. iv.—Histoire de la Mesure du Temps, 1802, vol. ii. pp. 32 and 33.

† See also Fig. 8. Plate IV. of the Supplément au Traité des Horloges Marines.

 The

Description of Arnold's escapement. He takes the wheel, detent, and pallet of Le Roy's first escapement, and unlocks, by a pallet, without recoil, as in Le Roy's second escapement; but the form and dimensions of the parts are very different; and the teeth of the wheel have a peculiar form; the detent moves by a spring joint instead of pivots, and it has an unlocking spring.

The teeth of the escape wheel (Fig. 12, Plate IX.) are of a cycloidal shape,* in the face part which is intended for action, the section of which, with those of the two other sides, form a sort of mixed triangle. B B *d* represents the detent, which is formed of a flexible piece or spring, bending between C and N; and in the part N B *d*, which is stronger than the other, is fixed the locking pallet, *a*, opposite an adjusting screw F. The pallet, projecting below the spring detent, locks upon the interior angle of the tooth; suspending the motion of the escape wheel, and leaving the balance to vibrate free, as pointed out in the preceding escapements. The action of the spring detent (for the joint of the detent is itself a spring) presses the locking pallet against the screw, F, except at the time of unlocking the wheel. A very delicate spring, N *e*, called the discharging, or unlocking spring (and also the tender spring), is attached by one end, N, to the spring detent, CBNB *a*; and, passing under the adjusting screw, F, extends a little beyond the extremity, *d*, of the detent itself. HHH is a circular piece attached to the axis of the balance and, *o*, the discharging pallet. This pallet, when the balance is in motion from *e* to *d*, presses against the end of the discharging spring, *n e*; and, carrying it together with the locking spring, B B *d*, disengages the locking piece, *a*, out of the internal angle of the tooth, with which it was in contact; and the escape wheel then communicates a new power to the balance, by its impulse on a pallet, *m*, which is fixed, or set, in the aperture of the circular piece. As soon as this is done, the spring detent, or locking spring, falls back to its position against the adjusting screw, F; and the pallet, by receiving or intercepting the next tooth, stops the motion of the escape wheel. When the balance returns from *d* to *e*, the unlocking pallet acts again on the extremity of the discharging spring, but, this being very delicate, gives way without disturbing the detent or locking spring; and the balance, after suffering a trifling degree of resistance by that contact, continues its free vibrations. At the next vibration, the unlocking takes place;

* As the descriptions of the escapements for which Arnold and Earnshaw have been rewarded, are not of considerable length in the present interesting communication, I have re-engraved the sketches, instead of referring to Plates 13, vol. xiii. and 2, 3, vol. xiv, of our Journal, where the full descriptions are given.—W. N.

place; and the action of the escapement proceeds successively, as explained before.

The detached escapement used by Mr. Earnshaw is represented in Fig. 13, which is taken from his statement, presented to the Board of Longitude. This escapement is similar to that of Arnold's, already described, except in small variations, which will be easily perceived, on a comparison of the two figures. It is besides asserted, and it appears probable from every circumstance relative to these constructions, that the late Mr. Arnold had made use of this form of escapement long before Mr. Earnshaw, but that he laid it aside, in order to adopt the escapement with cycloidal teeth, which he esteemed far preferable. In the escapement we are now considering, the escape wheel is shaped as appears in the figure; and, on the inspection of this representation, it will be easily observed, that the teeth presenting a right line, and escaping by a sharp point, their action cannot be so smooth, and the wear of the whole must be greater, than in the construction with protuberant cycloidal teeth. The detent is of the same kind as the other, and only differs from it, in being stopped by the inside of the head of the adjusting screw, instead of the extremity of the screw itself, and unlocking outwards, and not towards the centre.

Earnshaw's escapement, —is in effect the same, and in form nearly the same as Arnold's, —who used it before him: —it is more subject to wear.

The two constructions, which may be considered as the same, differ from the French detached escapements, such as those of F. Berthoud, which we have already explained, in the detent. In the new detent, the pivots are abolished, and the centre of motion is established in the locking piece itself; which, for that purpose, is made flexible near the extremity by which it is fixed. The elasticity of the detent, or locking piece, supplies also the office of the auxiliary spring placed at *a* or *u* (Fig. 9 and 10), or the spiral spring, which has been sometimes applied to the axis of the pivots, to keep the detent in the proper situation.

The escapements adopted by Arnold and Earnshaw are not different from those of the French, except in the spring joint of the detent instead of pivots.

The pivots of the old detent are so slender, that its performance cannot be supposed subject to any considerable degree of friction; and watches, with that kind of detent, have been known to go very well. Some able artists, upon that account, think, that the new detent is only preferable to the other, because it saves work, and is less expensive; but while the spring detent is allowed to perform as well, if not better, than the

Probability that the spring detent may be superior to that with pivots, in cheapness only: —but it is as good, if not better, and

consequently preferable.

the detent with pivots, which its universal use in this country seems to prove, that property, combined with the economy in the manufactory, must secure to the mechanism in question, the character of an improvement in the construction of time-keepers.

The spring detent appears to be the invention of Arnold, though Earnshaw claims it.

To whom are we indebted for the invention of the spring detent? The general opinion attributes it to the late Mr. Arnold; and we do not see any reason of sufficient weight to refuse him that merit. Mr. Earnshaw has claimed it in his own favour; but Mr. Arnold's labours have, at least, the advantage of priority; and the strength of this advantage, not having been done away by any proofs, which in our opinion can be esteemed satisfactory, must decide, our judgment in the present case, as in the like controversies upon other points, which have been considered in the course of this inquiry. The contrivance of the locking spring, or spring detent, therefore, appears to us to be due to the late Mr. Arnold. With regard to this mechanism, it is also worthy of remark, that the invention is entirely English, not a single passage existing in the writings of the French authors, by which any one of them might claim it with reason, or even plausibility. The first mention of any thing like the locking spring, to be found in foreign publications, is the detent without pivots, given by F. Berthoud in his *Supplément au Traité des Horloges Marines* (Fig. 6, Plate IV.); but that book was published in 1787, that is five years after Mr. Arnold had taken out his patent, and when many watches upon that construction had been in circulation. We cannot, therefore, allow him the credit of this thought; nor do we find, that other French artists have availed themselves of that hint, to carry the spring detent to the great degree of simplicity, which it has attained in this country.

It is not a foreign invention.

The modern discovery, or re-discovery, of the isochronism of the vibrations by the balance spring.

A little after the invention of the detached escapement, the isochronism of the vibrations of the balance, by means of the spiral spring, was, if not newly discovered, at least perfected and brought into general notice; and that principle added a great value to the detached escapement, while this mechanism secured the utility of the principle, by offering the species of insulated balance which it required. From some theories and experiments long known to the world, it would appear that the

vibration

vibration of a spring might be always supposed of equal duration;* and that advantage Dr. Hooke asserted himself to have attained with his invention in watches, which had been shewn to several persons.† The principle, however, could not be generally trusted, according to Dr. Hooke himself, who, in the Postscript to his Description of Helioscopes (p. 29), declares *that he had explained how the vibrations might be so regulated, as to make their durations either all equal, or the greater, slower, or quicker, than the less, and that in any proportion assigned.* We must suspect that these ideas were not properly digested, or regret that their communication by the author, in his lectures in Gresham College, was not sufficiently explicit to give precise rules for practice, and fix the attention of watchmakers upon the subject. After those hints, the principle seems to have been very little attended to for many years, and the isochronism was frequently attempted to be effected by means of mechanical contrivances in the escapement. Harrison endeavoured to accomplish that important object by the form of the back of the pallets; and on the return

Dr. Hooke's claim to the invention, doubtful.

Contrivances in the escapement to make all vibrations equal in time.

* Dr. Hooke De Potentia Restitiva.—S. Gravesandè's Natural Philosophy, by Desaguliers, 1747. v. i. p. 317, &c.

Dr. Hooke, in the Postscript to his Description of Helioscopes, gives (Tabula III.) a short communication of the general ground of his inventions for pocket watches, in the "Universal and "real Character" of Bishop Wilkins. This table has been republished in the quarto series of Nicholson's Journal, but we do not yet possess a translation of it. Dr. Hooke concludes the postscript above mentioned, with a *decimate* of the *centesme* of the inventions he intended to publish, and subjoins anagrams to some of them. Those which relate to watches, are the two following:

"1. A way of regulating all sorts of watches or timekeepers, so "as to make any way to equalize, if not exceed the *pendulum* "*clocks* now used." But there is no addition of any kind to this article.

"3. The true theory of *elasticity* or *springiness;* and a particu-"lar explication thereof, in several subjects in which it is to be "found: And the way of computing the velocity of bodies moved "by them, *c e i i i n o s s s t t u u.*" Which anagram, in the "Lec-"ture De Potentia Restitiva," is thus explained,—*Ut vis, sic tensio.*

† De Potentia Restitiva, p. 5.

of

of the voyage to Jamaica, added for the same purpose the cycloidal pin, to regulate the balance spring; but this method of adjustment never appeared satisfactory or certain. P. le Roy, in his *Mémoire sur la meilleure Manière de mesurer le Temps en Mer*, rewarded in 1766, first announced distinctly the discovery of a general principle, proper to produce the isochronism, by means of the balance spring, which is expressed as follows: *That in every spring sufficiently long, a certain portion of it will be isochronal, whether long or short; that the length of this portion being found, if it be lessened, the long vibrations will be quicker than the short ones; and that on the contrary, if the length be increased, the small arcs will be performed in less time than the great arcs.* This important property of the spring, enabled P. le Roy to bring to a happy issue his labours for the improvement of chronometry; and the art is indebted to him for the practical utility of that discovery, as much as for the invention of the detached escapement.

Peter le Roy first distinctly announced that isochronism is producible by the spring.

He asserted, that a given length of spring will be isochronal; a less length will have the wider vibrations quicker, and a greater length the narrower vibrations quicker.

F. Berthoud appears again on this occasion as a rival of P. le Roy, and arrogates to himself the honour of the discovery of the isochronism, by means of the balance spring; but his proofs are unsatisfactory, and the dates of the respective labours of those authors are too well established to admit of any presumption favourable to his pretensions. F. Berthoud did not publish any researches, or even ideas, upon the subject, till 1773, which is the date of the *Traité des Horloges Marines;* where (Premiere Partie, chap. iv. art. ii.), with the same want of candour, as we have already remarked in the case of the detached escapement, he gives a very minute detail of his own inquiries and experiments, without even once mentioning those of his predecessor. When that author was, afterwards, obliged to take notice* of the accusations published against him by P. le Roy (in his *Précis des differentes Recherches qui ont été faites depuis plus de quarante Ans, pour parvenir à resoudre le fameux Problème des Longitudes par le Secours de l'Horlogerie)*, he refers to a passage of the *Essai sur l'Horlogerie* (vol. i. p. 168), to shew that he had, in 1763, laid the foundation of his discovery; but that passage signifies nothing, and can

F. Berthoud claims the discovery,

—but without any proof or probability.

Berthoud's defence in answer to le Roy.

—is unsatisfactory.

* In the Eclaircissements, &c. servant de suite à l' Essai sur l' Horlogerie, et au Traité des Horloges Marines, 1773.

only

only prove that he intended to make experiments on the wide and narrow vibrations of the balance; and falls short of the hints contained in Dr. Hooke's Postscript, which we have already quoted. But though we cannot allow Berthoud the credit of originality, it is impossible to deny that his researches possess an eminent share of merit; and we have no doubt but that their publication has been of great service to artists, in that essential part of the construction of timekeepers. Berthoud found that the spiral spring, in order to be isochronal, must have an ascending force in arithmetical progression, and that this property may be effected, not only by the length of the spring, but by the number of coils, and the tapering or decreasing thickness from the centre to the extremity, &c. He adds besides the proportions of the tapering in many springs, which he had actually tried, and gives minute accounts of the experiments made with them in several timekeepers.

But the researches and labours of Berthoud are of great value, and importance.

He discovered the law of the force of an isochronal spring, and the causes which affect it.

The late Mr. Arnold applied to the balance the cylindrical or helical spring, which had been employed long before to a variety of purposes instead of the spiral, which had been constantly used in watches since the time of Dr. Hooke and M. Huygens.* This is one of the articles of his patent of 1782, and the specification is contained in these words: "*The in-*" "*curvating of the ends of the helical spring is attended with*" "*the property of rendering all the vibrations of equal duration,*" "*because the figure is always similar to itself.*" Whence it would appear, that provided the spring be made of that form, the vibrations cannot fail to be isochronal; but experience is contrary to that notion, and artists are obliged to attend to a variety of circumstances in the application of the helical, as well as that of the spiral spring. Mr. Arnold was asked by the Committee of the House of Commons, to which the petition of Mr. Mudge was referred,† "*What objections are there*" "*against the common spiral spring?*" To which he answered, "*That it is never a spiral, but when it is at rest; for the*"

Arnold applied the cylindrical or helical spring to chronometers, (asserting its superiority to the spiral).

He affirms that it is isochronal, if the fixed end be bended inwards: —but this is not the fact.

* One of Harrison's watches had an helical spring. See Earnshaw's Disclosure; who says the machine is now at Greenwich. W. N.

† Report from the Select Committee, &c. ordered to be printed, 11th June, 1793. p. 81.

 "*instant*

"*instant it begins to move, it assumes a figure that is not a* "*perfect spiral, both when the spring opens and closes, by the* "*contrary vibrations.*" But this explanation is not conclusive, and requires some modifications. A spring, of whatever form it may be, only acts when its figure suffers an alteration, and for that reason a relative change must take place, as well in the cylindrical as the spiral, or any other spring. What is the shape most favourable to isochronism, is another question to be decided by very delicate experiments, which we do not know to have been ever made. At present, some watchmakers think that the helical spring does not possess any advantage with regard to that property; but as the opinion of other persons is in the affirmative, while all the manufacturers, as far as our knowledge goes, agree in considering the cylindrical form as more easily managed than the other, its application seems entitled to the merit of a practical improvement.

What way be the best figure of a balance spring?

Arnold may be considered as the first applier of the helical spring (as an improvement).

That application* is ascribed by common report to Mr. Arnold, and we see no reasonable ground to dispute it to him, except the instance before noted, and the evidence of the late Josiah Emery, who declared before the Committee of the House of Commons, in the case of Mr. Mudge,† that he had read an account, in an English paper, of that sort of spring, under the name of cylindrical spring, about a year or two before Mr. Arnold took out his patent. This account was contained in an advertisement from Bow-street, relative to a number of watches that had been stolen in France and brought into England; but Mr. Arnold perhaps never saw it, and may have thought of the application of the helical spring to watches without previous hint or assistance.

Earnshaw denies the isochronism of the helical spring; and asserts, that he had discovered the remedy of tapering it:—but it was done long before by Berthoud.

Mr. Earnshaw, in the explanation of his timekeepers, presented to the Board of Longitude, after noticing the insufficiency of the cylindrical spring, states that he had, by long perseverance, found how to make springs increasing in thickness to the outer end, in order to effect the isochronism of the vibrations. This method of obtaining isochronal vibrations had been long before explained by Berthoud, with regard to the spiral spring, in that part of his Treatise on Marine Time-

* See note 2, p. 7.

† Report from the said Committee, pp. 104 and 105.

pieces

pieces which we have already quoted; and we are rather surprized to learn that it cost Mr. Earnshaw so much trouble to discover it; but, according to his account, that property does not answer completely the object in view, and watches, with isochronal springs, well adjusted at first, will progressively lose in their rates, from the relaxation which takes place in them. The remedy for that evil Mr. Earnshaw declares himself to have discovered, by a continuance of the same stubborn application which he had bestowed on the former part of his labours; and that it consists in making the springs of such a shape as to gain in the narrow vibrations, about five or six seconds per day more than in the wide ones. The reason of this contrivance is explained by the author in the following passage of his statement (p. 10): "I find the common relaxation of balance springs to be about five or six "seconds per day on their rates in the course of a year; "therefore, if the short vibrations are made by the shape of "the spring to go about that quantity faster than the long "ones, and as the spring relaxes in going by time, so the "watch accumulates in dirt and thickness of the oil, which "shortens the vibrations, the short ones then being quicker, "compensates for the evil of relaxation of the balance spring." But the whole of this explanation, though plausible at first sight, seems liable to considerable doubts and objections, and it would require a series of decisive experiments to prove the accuracy of the method, and the certainty of its application.

Gradual decay in the strength of springs, asserted by Earnshaw:

—who disclosed as a remedy that the narrow vibrations should be made the quickest.

This remedy doubtful.

Some skilful makers are not satisfied as to the reality of this relaxation of the balance spring, at least to the degree which is implied in the above reasoning; and none, we apprehend, will believe that the relaxation, supposing it to take place, will be so uniform as to admit of a remedy, fixed in all cases to the same quantity of difference between the wide and narrow vibrations. Is it probable that the effects of the relaxation, which proceeds from wear, can be ascertained by any other means than very long trials with each individual spring? Or is it to be supposed, that the progressive alteration of springs is of such a nature, as to be concluded from a short experiment, taking it for granted, that the variations will be always in proportion to the times? Some chronometers have been known

It may be questioned whether springs do fall off?

—or if so, whether uniformly?

—or otherwise determinable than by experiment?

Some chronometers gain on their rates;

known to accelerate instead of retarding, during a considerable time; and there are some which have gone for years without any material alteration in their rates; a fact which strongly militates against the rule proposed, as an unfailing and universal method. What would have been the effect of introducing into those timekeepers the invariable excess of five or six seconds of the narrow above the wide vibrations, which, like a quack medicine, is to cure, without distinction of symptoms or subjects, all the disorders proceeding from the cause of relaxation. The proposed remedy appears liable to the objection which naturally occurs on the slightest consideration of any compensation intended for circumstances which are contingent, and effects which cannot be anticipated. It is at first a source of irregularity, while there is no certainty of its proving an effectual correction of the supposed future changes. And again, even if the relaxation of the springs could be remedied by the shape, or any other essential requisite of the spring itself, it would still be hazardous to adopt a specific variation in that organ, on account of effects which might afterwards result from the different thickness of the oil or accumulation of dirt; two circumstances which are too variable and uncertain to appear susceptible of being counteracted by any regular process. Our arguments, however, ought to be allowed no other force than what can be derived from a clear general view of the subject; and we will admit, that, if Mr. Earnshaw should be able to produce a series of conclusive experiments in support of the accuracy of his suggestions, with certain rules for practice derived from them, his remedy for the relaxation of springs would then deserve to be reckoned a valuable addition to the art of making timepieces. In the mean time, the makers will do right to follow that method which is at present in general use among them, and continue their endeavours to accomplish the perfection of chronometers by the principle of isochronal vibrations.

and others continue steady.

Mr. Earnshaw's remedy is like an application of the same medicine to all patients,—to the healthy as well as the sick.

It includes the considerations of change in the oil or dirt in the machine; which are uncertain.

Mr. Earnshaw ought to exhibit facts and deductions; so as to give a practical rule.

—and till he does, his remedy is not advisable.

The isochronism of the balance affords the advantage of wide vibrations.

Among the great advantages resulting from the isochronism of the balance spring, is the facility which it affords to procure wide vibrations, and increase the power of the regulator. The irregularity proceeding from the springs, having their wide and narrow vibrations of different durations, was for a long time so great an obstacle to that practical improvement, that even the genius of Daniel Bernouilli, to whom we are indebted for very deep

deep researches upon the subject of chronometers, ascribed* the principal cause of the imperfections of watches to the wideness of the vibrations; and, while he assigned the small power of the balance, as the second cause of error, he found himself under the necessity of recommending † very short vibrations, in order to effect, in that way, the required equality of their duration.

This isochronism and the detached escapement do also destroy the irregularity arising from the first mover.

Another great advantage, proceeding from the isochronism of the balance spring, combined with the detached escapement, is the destruction of those variations which would arise from the main spring. From the first appearance of the invention of the detached escapement, we find P. le Roy attentive to that important object; and in 1748, when he presented his model to the Academy of Sciences, he tried it before that assembly with the main spring, at the two extremes, when the difference in the rate of going was found to be very small.‡ The author shewed how to destroy even that small difference, and explained how all the vibrations might be rendered of the same duration.|| P. le Roy having afterwards discovered the method, of effecting the isochronism, by means of the balance spring, trusted so confidently to that property, combined with his improved detached escapement, that in the chronometer for which he was rewarded in 1766, he laid aside the chain and fusee, and made the maintaining power act immediately on the train of wheels. In the "Succinct Account of the Attempts "of Messrs. Harrison and Le Roy, for finding the Longitude "at Sea, by P. le Roy;" an English translation of which, by a Fellow of the Royal Society, was published in 1768, in London; the author expressly states, that his watches, according to that construction, go thirty hours without winding up; and upon examining their rate of going in the first, and in the last fifteen

Peter le Roy, who had produced both so very early,

—did reject the chain and fusee, in 1766.

* Recherches, Mechaniques et Astronomiques, sur la Question proposée par l' Academie Royale des Sciences, pour l' Année, 1745: la meilleure Maniere de trouver l' Heure en Mer. § xxii.

† The same. § xxviii.

‡ Journal des Machines, 1748.

|| See the description of that chronometer, in the Mémoire sur la meilleure Maniere de mesurer le Temps en Mer, par P. le Roy; attached to Cassini's voyage.

hours

An experiment of this kind afterwards made by Arnold.

hours, no sensible variation is perceptible. The late Mr. Arnold also made a similar experiment at the house of the late Mr. Aubert, and in the presence of several ingenious gentlemen; * and the result evinced the success with which he had removed, by the same means, the errors arising from the inequalities of the maintaining power.

Concluding remarks.

The preceding statement is intended to afford a candid, though not a minute account of the progress of chronometry to the present day; as far as the steps, by which the art has advanced to its actual state of perfection, may be esteemed either inventions entirely new, or essential improvements in the methods known before. Those methods which do not admit of philosophical or clear description, and also such as have not yet been sufficiently established by experiment, are not considered as within the limits of this sketch; and we must observe the same silence with regard to improvements in mere workmanship, though in themselves of great practical value.

The mechanical problem of the longitude is greatly indebted to the skill of workmen;

We cannot, however, avoid noticing, that the leading principles, by which the mechanical solution of the problem of the longitude at sea has been effected, have derived a considerable part of their utility and success from the great perfection in the execution, which is a consequence of the flourishing state of the trade of watchmaking; and, under that point of view, we must thank the great wealth and maritime commerce of this country, for an extensive demand, which has not only promoted the manufactory of good watches, but created a new branch of trade, by a considerable demand for timekeepers for sea.

—who have been supported by our extensive commerce, &c.

II.

Account of a Series of Experiments, shewing the Effects of Compression in modifying the Effects of Heat. By Sir James Hall, *Bart. F. R. S. Edinburgh, &c. &c.*

(Concluded from page 212.)

General obser-

WHAT has been said of the heat conveyed by internal vol-

* That experiment is generally known, and has been recently mentioned in the Answer to Mr. Earnshaw, by Mr. Dalrymple. p. 80.

canic

canic streams, applies equally to that deeper and more general heat by which the lavas themselves are melted and propelled upwards. That they have been really so propelled, from a great internal mass of matter, in liquid fusion, seems to admit of no doubt, to whatever cause we ascribe the heat of volcanoes. It is no less obvious, that the temperature of that liquid must be of far greater intensity than the lavas, flowing from it, can retain when they reach the surface. Independently of any actual eruption, the body of heat contained in this vast mass of liquid, must diffuse itself through the surrounding substances, the intensity of the heat being diminished by slow gradations, in proportion to the distance to which it penetrates. When, by means of this progressive diffusion, the heat has reached an assemblage of loose marine deposites, subject to the pressure of a great superincumbent weight, the whole must be agglutinated into a mass, the solidity of which will vary with the chemical composition of the substance, and with the degree of heat to which each particular spot has thus been exposed. At the same time, analogy leads us to suppose, that this deep and extensive heat must be subject to vicissitudes and intermissions, like the external phenomena of volcanoes. We have endeavoured to explain some of these irregularities, and a similar reasoning may be extended to the present case. Having shewn, that small internal streams of lava tend successively to pervade every weak part of a volcanic mountain, we are led to conceive, that the great masses of heated matter just mentioned, will be successively directed to different parts of the earth; so that every loose assemblage of matter, lying in a submarine and subterranean situation, will, in its turn, be affected by the indurating cause; and the influence of internal volcanic heat will thus be circumscribed within no limits but those of the globe itself.

vations on the operation and extent of the heat which is known to act in the interior of our globe.

A series of undoubted facts prove, that all our strata once lay in a situation similar in all respects to that in which the marine deposites just mentioned have been supposed to lie.

All our strata were once beneath the sea.

The inhabitant of an unbroken plain, or of a country formed of horizontal strata, whose observations have been confined to his native spot, can form no idea of those truths, which at every step in an alpine district force themselves on the mind of a geological observer. Unfortunately for the progress of geology, both London and Paris, are placed in countries of little interest;

interest; and those scenes by which the principles of this science are brought into view in the most striking manner, are unknown to many persons best capable of appreciating their value. The most important, and at the same time, the most astonishing truth which we learn by any geological observations, is, that rocks and mountains now placed at an elevation of more than two miles above the level of the sea, must at one period have lain at its bottom. This is undoubtedly true of those strata of limestone which contain shells; and the same conclusion must be extended to the circumjacent strata. The imagination struggles against the admission of so violent a position; but must yield to the force of unquestionable evidence; and it is proved by the example of the most eminent and cautious observers, that the couclusion is inevitable.*

That the mountains have been elevated out of the sea, is much more probable than that the sea subsided from them.

Another question here occurs, which has been well treated by Mr. Playfair. Has the sea retreated from the mountains? or have they risen out of the sea? He has shewn, that the balance of probability is incomparably in favour of the latter supposition; since, in order to maintain the former, we must dispose of an enormous mass of sea, whose depth is several miles, and whose base is greater than the surface of the whole sea. Whereas the elevation of a continent out of the sea like ours, would not change its level above a few feet; and even were a great derangement thus occasioned, the water would easily find its level without the assistance of any extraordinary supposition. The elevation of the land, too, is evinced by what has occasionally happened in volcanic regions, and affords a complete solution of the contortion and erection of strata, which are almost universally admitted to have once lain in a plane and horizontal position.

Whatever opinion be adopted as to the mode in which the land and the water have been separated, no one doubts of the ancient submarine situation of the strata.

They were originally covered with other earth.

An important series of facts proves, that they were likewise subterranean. Every thing indicates that a great quantity of matter has been removed from what now constitutes the surface of our globe, and enormous deposites of loose fragments, evidently detached from masses similar to our common rock, evince

* Saussure, Voyages dans les Alpes, tom. ii. p. 99—104.

the

the action of some very powerful agent of destruction. Analogy too, leads us to believe, that all the primary rocks have once been covered with secondary; yet, in vast districts, no secondary rock appears. In short, geologists seem to agree in admitting the general position, that very great changes of this kind have taken place in the solid surface of the globe, however much they may differ as to their amount, and as to their causes.

Dr. Hutton ascribed these changes to the action, during very long time, of those agents, which at this day continue slowly to corrode the surface of the earth; frosts, rains, the ordinary floods of rivers, &c. which he conceives to have acted always with the same force, and no more. But to this opinion I could never subscribe, having early adopted that of Saussure, in which he is joined by many of the continental geologists. My conviction was founded upon the inspection of those facts in the neighbourhood of Geneva, which he has adduced in support of his opinion. I was then convinced, and I still believe, that vast torrents, of depth sufficient to overtop our mountains, have swept along the surface of the earth, excavating vallies, undermining mountains, and carrying away whatever was unable to resist such powerful corrosion. If such agents have been at work in the Alps, it is difficult to conceive that our countries should have been spared. I made it therefore my business to search for traces of similar operations here. I was not long in discovering such in great abundance; and, with the help of several of my friends, I have traced the indications of vast torrents in this neighbourhood, as obvious as those I formerly saw on Saleve and Jura. Since I announced my opinion on this subject, in a note subjoined to my paper on Whinstone and Lava, published in the fifth volume of the Transactions of this Society, I have met with many confirmations of these views. The most important of these are derived from the testimony of my friend Lord Selkirk, who has lately met with a series of similar facts in North America.

—which is thought to have been weathered and washed away, &c.

—but there are indications of vast torrents having swept it away.

It would be difficult to compute the effects of such an agent; but if, by means of it, or of any other cause, the whole mass of secondary strata, in great tracts of country, has been removed from above the primary, the weight of that mass alone must have been sufficient to fufil all the conditions of the Huttonian Theory,

This superincumbent weight would produce a compression sufficient to fulfil the Huttonian Theory.

Theory, without having recourse to the pressure of the sea. But when the two pressures were combined, how great must have been their united strength!

We are authorized to suppose, that the materials of our strata, in this situation, underwent the action of fire. For volcanoes have burnt long before the earliest times recorded in history, as appears by the magnitude of some volcanic mountains; and it can scarcely be doubted, that their fire has acted without any material cessation ever since the surface of our globe acquired its present form. In extending that same influence to periods of still higher antiquity, when our strata lay at the bottom of the sea, we do no more than ascribe permanence to the existing laws of nature.

The combination of heat and compression resulting from these circumstances, carries us to the full extent of the Huttonian Theory, and enables us, upon its principles, to account for the igneous formation of all rocks from loose marine deposites.

Enumeration of the effects of heat under this pressure in producing the various stony matters now found.

The sand would thus be changed to sandstone; the shells to limestone; and the animal and vegetable substances to coal.

Other beds, consisting of a mixture of various substances, would be still more affected by the same heat. Such as contained iron, carbonate of lime, and alkali, together with a mixture of various earths, would enter into thin fusion, and, penetrating through every crevice that occurred, would, in some cases, reach what was then the surface of the earth, and constitute lava: in other cases, it would congeal in the internal rents, and constitute prophyry, basalt, greenstone, or any other of that numerous class of substances, which we comprehend under the name of *whinstone*. At the same time, beds of similar quality, but of composition somewhat less fusible, would enter into a state of viscidity, such as many bodies pass through in their progress towards fusion. In this state, the particles, though far from possessing the same freedom as in a liquid, are susceptible of crystalline arrangement;* and the substance, which,

* This state of viscidity, with its numberless modifications, is deserving of great attention, since it affords a solution of some of the most important geological questions. The mechanical power exerted by some substances, in the act of assuming a crystalline form, is

which, in this sluggish state, would be little disposed to move, being confined in its original situation by contiguous beds of more refractory matter, would crystallize, without undergoing any change of place, and constitute one of those beds of whinstone which frequently occur interstratified with sandstone and limestone.

In other cases where the heat was more intense, the beds of sand, approaching more nearly to a state of fusion, would acquire such tenacity and toughness, as to allow themselves to be bent and contorted, without laceration or fracture, by the influence of local motions, and might assume the shape and character of primary schistus: the limestone would be highly crystallized, and would become marble, or, entering into thin fusion, would penetrate the minutest rents in the form of calcareous spar. Lastly, when the heat was higher still, the sand itself would be entirely melted, and might be converted, by the subsequent effects of slow cooling, into granite, sienite, &c.; in some cases, retaining traces of its original stratification, and constituting gneiss and stratified granite; in others, flowing into the crevices, and forming veins of perfect granite.

The manner in which the strata were most probably heaved or raised up.

In consequence of the action of heat, upon so great a quantity of matter, thus brought into a fluid or semifluid state, and in which, notwithstanding the great pressure, some substances would be volatilized, a powerful heaving of the superincumbent mass must have taken place; which, by repeated efforts, succeeding each other from below, would at last elevate the strata into their present situation.

The Huttonian Theory embraces so wide a field, and comprehends the laws of so many powerful agents, exerting their

is well known. I have seen a set of large and broad crystals of ice, like the blade of a knife, formed in a mass of clay, of such stiffness, that it had been just used to makes cups for chemical purposes. In many of my former experiments, I found that a fragment of glass made from whinstone or lava, when placed in a muffle heated to the melting point of silver, assumed a crystalline arrangement, and underwent a complete change of character. During this change, it became soft, so as to yield to the touch of an iron rod; yet retained such stiffness, that, lying untouched in the muffle, it preserved its shape entirely; the sharp angles of its fracture not being in the least blunted.

 influ-

influence in circumstances and in combinations hitherto untried, that many of its branches must still remain in an unfinished state, and may long be exposed to partial and plausible objections, after we are satisfied with regard to its fundamental doctrines. In the mean time I trust, that the object of our pursuit has been accomplished, in a satisfactory manner, by the fusion of limestone under pressure. This single result affords, I conceive, a strong presumption in favour of the solution which Dr. Hutton has advanced of all the geological phenomena; for, the truth of the most doubtful principle which he has assumed, has thus been established by direct experiment.

APPENDIX.

No. I.

Specific Gravity of some of the foregoing Results.

Concerning the specific gravities of bodies; particularly such as are porous: the aggregate being less heavy, specifically, than the parts separately taken.

As many of the artificial limestones and marbles produced in these experiments, were possessed of great hardness and compactness, and as they had visibly undergone a great diminution of bulk, and felt heavy in the hand, it seemed to me an object of some consequence to ascertain their specific gravity, compared with each other, and with the original substances from which they were formed. As the original was commonly a mass of chalk in the lump, which, on being plunged into water, begins to absorb it rapidly, and continues to do so during a long time, so as to vary the weight at every instant, it was impossible, till the absorption was complete, to obtain any certain result; and to allow for the weight thus gained, required the application of a method different from that usually employed in estimating specific gravity.

In the common method, the substance is first weighed in air, and then in water; the difference indicating the weight of water displaced, and being considered as that of a quantity of water equal in bulk to the solid body. But as chalk, when saturated with water, is heavier, by about one-fourth, than when dry, it is evident, that its apparent weight, in water, must be increased, and the apparent loss of weight diminished exactly to that

that amount. To have a just estimate, then, of the quantity of water displaced by the solid body, the apparent loss of weight must be increased by adding the absorption to it.

Two distinct methods of taking specific gravity thus present themselves, which it is of importance to keep separate, as each of them is applicable to a particular class of subjects.

One of these methods, consists in comparing a cubic inch of a substance in its dry state, allowing its pores to have their share in constituting its bulk, with a cubic inch of water.

The other depends upon comparing a cubic inch of the solid matter of which the substance is composed, independently of vacuities, and supposing the whole reduced to perfect solidity, with a cubic inch of water.

Thus, were an architect to compute the efficacy of a given bulk of earth, intended to load an abutment, which earth was dry, and should always remain so, he would undoubtedly follow the first of these modes: Whereas, were a farmer to compare the specific gravity of the same earth with that of any other soil, in an agricultural point of view, he would use the second mode, which is involved in that laid down by Mr. Davy.

As our object is to compare the specific density of these results, and to ascertain to what amount the particles have approached each other, it seems quite evident that the first mode is suited to our purpose. This will appear most distinctly, by inspection of the following table, which has been constructed so as to include both.

TABLE OF SPECIFIC GRAVITIES.

I.	II. Weight in air, dry.	III. Weight in water.	IV. Weight in air, wet.	V. Difference between columns II. & III.	VI. Difference between columns II. & IV. or absorption	VII. Absorption *per cent.*	VIII. Sum of columns V. and VI.	IX. Specific gravity by common mode.	X. Specific gravity by new mode.
1.	125.90	77.55	135.65	47.35	9.75	7.74	57.10	2.604	2.204
2.	9.94	6.13	9.99	3.81	0.05	0.50	3.86	2.609	2.575
3.	15.98	9.70	16.02	6.28	0.04	0.25	6.32	2.544	2.528
4.	5.47	3.33	5.48	2.14	0.01	0.18	2.15	2.556	2.544
5.	18.04	10.14	18.06	7.90	0.02	0.11	7.92	2.283	2.277
6.	6.48	3.74	7.10	2.74	0.62	9.56	3.36	2.365	1.928
7.	10.32	5.97	10.36	4.35	0.04	0.39	4.39	2.372	2.350
8.	54.57	31.30	55.23	23.27	0.66	1.21	23.93	2.345	2.280
9.	72.27	41.10	76.13	31.17	3.86	5.34	35.03	2.318	2.063
10.	37.75	21.15	38.30	16.60	0.55	1.45	17.15	2.274	2.201
11.	21.21	12.55	21.26	8.66	0.05	0.24	8.71	2.449	2.435
12. Marble.	18.59	11.56	18.61	7.03	0.02	0.18	7.05	2.644	2.636
13. Chalk.	504.15	302.40	623.20	201.75	119.05	23.61	320.80	2.498	1.571
14. Average Chalk.	444.30	264.35	550.80	179.95	106.50	23.97	286.45	2.469	1.551
15. Rammed Powder.	283.97	—	—	—	—	—	198.65	—	1.429

Expla-

Explanation.

Explanation of the table of specific gravities, &c.

Column I. contains the number affixed to each of the specimens, whose properties are expressed in the table.

The first eleven are the same with those used in the paper read in this Society on the 30th of August 1804, and published in Nicholson's Journal for October following, and which refer to the same specimens. No. 12. Is a specimen of yellow marble, bearing a strong resemblance to No. 3. No. 13. A specimen of chalk. No. 14. Shews the average of three trials with chalk. No. 15. Some pounded chalk, rammed in the manner followed in these experiments. In order to ascertain its specific gravity, I rammed the powder into a glass-tube, previously weighed; then, after weighing the whole, I removed the chalk, and filled the same tube with water. I thus ascertained, in a direct manner, the weight of the substance, as stated in column II., and that of an equal bulk of water, stated in column VIII.

Column II. Weight of the substance, dry in air, after exposure, during several hours to a heat of 212° of Fahrenheit.

Column III. Its weight in water, after lying long in the liquid, so as to perform its full absorption; and all air-bubbles being carefully removed.

Column IV. Weight in air, wet. The loose external moisture being removed by the touch of a dry cloth; but no time being allowed for evaporation.

Column V. Difference between columns II. and III. or apparent weight of water displaced.

Column VI. Difference between columns II. and IV., or the absorption.

Column VII. Absorption reduced to a *per centage* of the dry substance.

Column VIII. Sum of columns V. and VI., or the real weight of water displaced by the body.

Column IX. Specific gravity, by the common mode, resulting from the division of column II. by column V.

Column X. Specific gravity, in the new mode, resulting from the division of column II. by column VIII.

The Specific gravities ascertained by the new mode, and expressed in column X. correspond very well to the idea which is formed of their comparative densities, from other circumstances,

Explanation of the table of specific gravities, &c.

stances, their hardness, compact appearance, susceptibility of polish, and weight in the hand.

The case is widely different, when we attend to the results of the common method contained in column IX. Here the specific gravity of chalk is rated at 2.498, which exceeds considerably that of a majority of the result tried. Thus, it would appear, by this method, that chalk has become lighter by the experiment, in defiance of our senses, which evince an increase of density.

This singular result arises, I conceive, from this, that, in our specimens, the faculty of absorption has been much more decreased than the porosity. Thus, if a piece of crude chalk, whose specific gravity had previously been ascertained by the common mode, and then well dried in a heat of 212°, were dipped in varnish, which would penetrate a little way into its surface; and, the varnish having hardened, the chalk were weighed in water, it is evident that the apparent loss of weight would now be greater by 23.61 *per cent.* of the dry weight, than it had been when the unvarnished chalk was weighed in water; because the varnish, closing the superficial pores, would quite prevent the absorption, while it added but little to the weight of the mass, and made no change on the bulk. In computing, then, the specific gravity, by means of this last result, the chalk would appear very much lighter than at first, though its density had, in fact, been increased by means of the varnish.

A similar effect seems to have been produced in some of these results by the agglutination or partial fusion of part of the substance, by which some of the pores have been shut out from the water.

This view derives some confirmation from an inspection of columns VI. and VII.; the first of which expresses the absorption; and the second, that result, reduced to a *per centage* of the original weight. It there appears, that whereas chalk absorbs 23.97 *per cent.*, some of our results absorb only 0.5, or so low as 0.11 *per cent.* So that the power of absoption has been reduced from about one-fourth, to less than the five hundredth of the weight.

I have measured the diminution of bulk in many cases, particularly in that of No. 11. The chalk, when crude, ran to th[illegible]th degree of Wedgwood's gage, and shrunk so much during the experiment, that it ran to the 161st; the difference amount-

amounting to 86 degrees. Now, I find, that Wedgwood's gage tapers in breadth, from 0.5 at zero of the scale, to 0.3 at the 240th degree. Hence, we have for one degree 0.000833. Consequently, the width, at the 75th degree, amounts to 0.437525; and at the 161st, to 0.365887. These numbers, denoting the linear measure of the crude chalk, and of its result under heat and compression, are as 100 to 83.8; or, in solid bulk, as 100 to 57.5. Computing the densities from this source, they are as 1 to 1.73. The specific gravities in the Table, of the chalk, and of this result, are as 1.551 : 2.435; that is, as 1 to 1.57. These conclusions do not correspond very exactly; but the chalk employed in this experiment, was not one of those employed in determining average specific gravity in the Table; and other circumstances may have contributed to produce irregularity. Comparing this chalk with result second, we have 1.551 : 2.575 so 1 : 1.6602.

Explanation of the Table of specific gravities, &c.

No. II.—TABLE,

CONTAINING THE REDUCTION OF THE FORCES MENTIONED IN CHAP. VII. TO A COMMON STANDARD.

I. Number of experiment referred to in Chap. VII.	II. Bore, in decimals of an inch.	III. Pressure in hundred weights.	IV. Temperature by Wedgwood's pyrometer.	V. Depth of sea in feet.	VI. Ditto in miles.	VII. Pressure, expressed in atmospheres.
1	0.75	3	22	1708.05	0.3235	51.87
2	0.75	3	25	1708 05	0.3235	51.87
3	0.75	10	20	5693.52	1.0783	172.92
4	0.75	10	31	5693.52	1.0783	172.92
5	0.75	10	41	5693.52	1.0783	172 92
6	0.75	10	51	5693.52	1.0783	172.92
7	0.75	10	—	5693 52	1.0783	172.92
8	0.54	2	—	2196.57	0.4160	66.71
9	0.54	{ 4	—	4393.14	0.8320	133.43
		{ 8.1	—	8896.12	1.6848	270.19
10	0.75	3	21	1708.05	0.3235	51.87
11	0.75	4	25	2277.41	0 4313	69.70
12	0.75	5	—	2846.76	0.5396	86.46

Explanation.

Explanation of the Table of forces, &c.

Column I. contains the number of the experiment, as referred to in the text. Column II. The bore of the barrel used, in decimals of an inch. Column III. The absolute force applied to the barrel, in hundred-weights. Column IV. The temperature, in Wedgwood's scale. Column V. The depth of sea at which a force of compression would be exerted equal to that sustained by the carbonate in each experiment, expressed in feet. Column VI. The same in miles. Column VII. Compressing force, expressed in atmospheres.

Both Tables were computed separately, by a friend, Mr. J. Jardine, and myself.

The following data were employed:

Area of a circle of which the diameter is unity, 0.785398.

Weight of a cubic foot of distilled water, according to Professor Robison, 998.74 ounces avoirdupois.

Mean specific gravity of sea-water, according to Bladh, 1.0272.

Mean heighth of the barometer at the level of the sea 29.91196 English inches, according to Laplace.

Specific gravity of mercury, according to Cavendish and Brisson, 13.568.

III.

Catalogue of thirty-one Specimens, shewing the Result of Sir James Hall's *Experiments on the Effects of Heat modified by Compression; which were deposited by him in the British Museum, on the 29th of June* 1806.*

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

NUMBERS 1, 2, 3, 4, 5, 6, and 7, were all produced in separate experiments from pounded carbonate of lime.

Number 1, was obtained in 1799. It is a firm stone, requiring a smart blow of a hammer to break it. It was enclosed in a cartridge of paper, the mark of which it still bears. The other six are

* Communicated to the editor by the author.

are still harder and more compact, approaching nearly in these qualities to common limestone. Numbers 2, 4, and 7, possess a degree of semitransparency, most remarkable in Number 4. And all of these specimens exhibit an uneven fracture, approaching to that of beeswax and marble. Their colours are variously, though slightly, tinged with yellow and blue. In particular Number 3, which, though produced from common white chalk, resembles a yellow marble. Numbers 3, 5, and 6, have taken a tolerable polish. Number 7, contains a shell introduced along with the pounded chalk, and now closely incorporated with it. Along with Number 3, is a specimen (A. 3.) of common yellow marble, bearing a strong resemblance to the artificial stone.

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

Numbers 8, 9, 10, and 11, all formed from pieces of chalk exposed unbroken to heat and pressure. Number 8 is remarkable for a shining grain and semitransparency.

Numbers 9 and 10, shew parallel planes like internal stratification, which has often appeared in calk in consequence of the action of heat, though nothing of the kind could be seen in the native mass. Number 11, very compact, and of a yellow colour.

Numbers 12 and 13, examples of welding, in which the pounded chalk has been incorporated with a lump of chalk upon which it had been rammed, so that their joining is hardly visible in the fracture.

Numbers 14, 15, and 16, shewing the fusion of the carbonate well advanced, with a considerable action on the porcelain tube. In Number 15, the rod of chalk is half melted, and a yellow substance produced by a mixture of the carbonate with the porcelain. Number 16, is a lump of chalk in a state indicating softness, a piece of porcelain which lay in contact with it having sunk a little into the substance of the carbonate.

Numbers 17 and 18, and all the following numbers, being delicate, are enclosed in tubes of glass, and fixed with sealing-wax on little cups of wood. Number 17, formed from pounded chalk, shows in one part the most complete formation of spar, with its rhomboidal fracture, I have ever obtained. The carbonate having lost some of its carbonic acid, had crumbled so much in its essential parts by the action of the air, that the crystallization was no longer visible, and I had given up the

 specimen

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

specimen for lost, till some time in July 1804, when employed in examining these results, in order to show them in the Royal Society of Edinburgh, a mass of the carbonate broke in two, and exhibited the fracture now before us, nearly in as good a state as it was originally. I immediately enclosed it in a glass tube, and sealed it up with wax; so that I have hopes of preserving it; and it still continues intire, though now sealed up for a year and a half. Number 18, likewise from pounded chalk, is perfectly fresh and intire, though made more than two years ago; it shows some beautiful clear crystals of spar in parallel plates, but they are so small as to require the use of a glass.

Numbers 19, 20, and 21, shew examples of fusion and action on the tubes. In Number 19, a shell is finely united to some pounded chalk. In Number 20, the mass, originally of pounded chalk, is sinking upon itself, and acting at the same time upon the tube; the fracture of the carbonate, in its pure parts, shewing brilliant facettes of crystallization. In Number 21, the carbonate in a state like the last; the compound of porcelain and carbonate shewing its liquidity by penetrating the tube, so as to form a distinct view of a dark colour, and then spreading on its outside to a considerable extent, terminating with the black line, alluded to in the account of the experiments.

Numbers 22, 23, and 24, give proofs of intire fusion. In Number 22, we see two porcelain tubes, enclosed for preservation in a glass tube; the end attached to the little wooden cup must be held downwards, to show the position in which the experiment was made. The innermost porcelain tube stands with its muzzle upwards, and the outermost covers it in the inverse position. The carbonate was contained in the inner tube during the action of heat: the barrel failed suddenly, and the carbonate has boiled over the lips of the inner tube, running down, as here appears, almost to its bottom: thus proving, that immediately previous to the failure of the apparatus, the carbonate had been in a liquid state. Number 23, two mases of carbonate welded together in a complete state of froth. The substance shining, and semitransparent.

Number 24, two separate masses exposed together to heat; one from pounded chalk, now in a state quite like the last, the other put in as a lump of chalk dressed flat at both ends, and a letter cut at each end (as done in many of the experiments).

It

It is in a shining and almost transparent state; at one end the flat form and the letter are still visible; the other end is completely rounded in fusion, with a glassy surface.

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

Number 25, shews the substance produced by the combination of carbonate of lime with pure silex. Part of the porcelain tube in this specimen is filled with pounded silex; which having a very feeble agglutination, is supported by some sealing-wax. Upon the silex, during the experiment, had lain some carbonate of lime, the lower part of which had united with the silex, producing a semitransparent substance, with a delicate tinge of blue. The termination of this compound, as it had advanced downwards into the silex, shews the round and mammillated form of chalcedony.

Number 26, result of an experiment with heat and compression, made July 22, 1805, with some pure carbonate of lime, prepared by Mr. Hatchett. The carbonate was enclosed in a small tube of platina, and was thus secured against all contamination.

Number 27, result of an experiment made likewise in platina, with a fragment of a perriwinkle-shell: the form of the shell is still visible, though the substance is glazed by semifusion. Along with it, on the same stand, is a small drop like a pearl, formed by the intire fusion of one of the fragments; and a portion of a shell of the same kind, in its natural state, is introduced, in order to show what change had taken place during the experiment.

Number 28, is a specimen of coal produced from horn; it is a shining black substance, exactly resembling pitch, or black sealing-wax: it was formed in a low red-heat, and in circumstances of compression; by which, while some of the volatile parts of the original were allowed to separate, others were retained. It has thus acquired a jet-black colour, while it retains its inflammability, and burns with bright flame.

Number 29, likewise produced from an animal substance, flannel. In this case, none of the compound parts of the original substance seem to have separated from it, owing either to less heat or greater closeness, than in the last case. The consequence has been, that the original colour has undergone much less change, being of a yellow-red. At the same time the substance has been in a state of fusion, and has assumed a polish

Specimens of results of bodies exposed to strong heat under compression, by Sir James Hall.

polish by moulding itself on the glass into which it had been pressed. This result seems to bear some analogy to the substance called resinasphaltum, described by Mr. Hatchett.

Number 30, is a piece of wood, partially converted into coal by heat and compression. In some parts, the substance entirely resembles pitch, being full of large and shining air holes; in others, the fibres of the wood are still distinctly visible. The whole is jet-black, and burns with a bright flame.

Number 31, is a specimen of the substance like wool, formed in several of these experiments by the exudation of the fusible metal through the barrel of iron, the metal in a liquid state spouting to a considerable distance, and depositing this substance upon any obstacle opposed to the stream.

IV.

Enumeration of several Cases of Ships which have been struck by Lightning; the destructive Effects having acted in a vertical Direction towards the Centre of the Earth, and never horizontally. In a Letter from JAMES HORSBURGH, *Esq.*

To Mr. NICHOLSON.

SIR, *Walworth, July 2, 1806.*

Introductory letter.

I HAVE taken the liberty to enclose some remarks on a few ships that have been struck by lightning. I was induced to do so, because it has never come within the reach of my knowledge, that the yards of any vessel have been injured by lightning.

It may be probable, that the electric matter never acts in a horizontal direction, destructively. But I must own that I have little knowledge of the subject here mentioned. And it is principally to point out, how seldom the yards of ships are injured by lightning, that I have taken this liberty.* If you

* Authentic facts seldom fail to afford instructions to those sciences which are grounded immediately on experiment. That the yards of ships are seldom or never injured by lightning, appears to arise not from their horizontal position, but from their lying out of the circuit or immediate path of the lightning from the clouds to the earth.—W. N.

think

think any part of these remarks deserving of a place in your valuable Journal, you will greatly oblige

Your most obedient servant.
JAMES HORSBURGH.

P.S. It may be observed, that ships navigating in seas or places where lightning is frequent, should be careful to place no cargo of an inflammatory nature near the masts; for the electric matter is frequently conducted by them into the hold. This happened to the King George, in Canton River: and a few years ago, the Royal Charlotte, with all her crew, were blown to atoms: this happened at Diamond Harbour, in the river Hooghley, during a night when much thunder and lightning prevailed. She had a great quantity of gun-powder placed forward in the hold, (said to have been stowed close forward around the mast), and it was supposed that her foremast had been struck by lightning; which probably was conducted by the mast into the hold among the gun-powder, and produced the dreadful explosion, which was heard at a great distance, and the concussion felt several miles. Very few fragments of the ship were visible next morning.

Dreadful consequences of lightning, conducted by ships' masts.

Enumeration of Ships struck by Lightning, &c.

In June 1792, returning from China by the Mindora passage, in the Anna, when we were in latitude 13° N°, and about $2\frac{1}{2}$° from the west coast of Luconia, a squall of wind from the south-west was experienced, followed by heavy rain, with much thunder and lightning. At this time a loud explosion burst over the ship, the lightning first embracing * the pole of the main-top-gallant-mast, tore it and the mast into small fragments, in its passage downwards: then embracing the head of the top-mast in its descent, tore it into pieces. From the topmast, it continued its direction down the mainmast, on one side of the mast, tearing away the hounds, and injuring the mast greatly, particularly where there was any iron-work. About eight feet above the deck, the electric matter was attracted from the main-

The masts of the Anna destroyed or injured by lightning.

* I suppose the word "*embracing*" to denote the visible passage of the lightning, as it *surrounded* the parts here and elsewhere spoken of.—N.

mast

mast by a large iron thimble near it, which was fixed to the mizen-stay: this thimble it scorched black, and cut a part of the stay; from whence its direction was no longer precepttible. All the parts of the topmast and top-gallant-mast, which were scrapped and greased, were split into a thousand pieces; but neither the heads of these masts, nor the caps, where they were covered with blacking, received any injury. None of the yards which were fixed to the masts, four in number, received the smallest damage from the lightning, nor any of the sails touched by it. Four men which were sitting under the top, to shelter themselves from the rain, had the hair of their heads and eyebrows a little singed, but received no further injury. The colour of this lightning appeared to be a pure white.

The greased parts were shattered, but the blacked parts remained uninjured. The yards were not injured.

In June 1788, a snow, in Bombay Harbour, belonging to the Honourable East India Company, was struck by lightning. It embraced the main-topmast head, rent that mast in pieces, and the mainmast was split from head to heel, and rendered unserviceable; whilst the main, and main-topsail yards that were fixed aloft on the masts, did not receive any injury.

The main-masts of a snow destroyed, and not the yards.

In July or August 1792, a ship from Bombay, bound to China, was struck by lightning in Malacca Strait, near Prince of Wales Island. It embraced the masts forward, and destroyed them; but none of the yards which were fixed on these masts received any injury.

Similar events.

About September 1793, the King George, a large ship, belonging to Bombay, proceeding up Canton River, was struck by lightning. It embraced the fore-top-gallant-mast head, shivered the masts in the fore part of the ship, killed the people in the fore-top, and some of those on deck which stood near the foremast. Although the fore-topmast and top-gallant-mast were much perforated by the lightning, and in danger of tumbling down, none of the yards were injured. The electric matter was conducted by the foremast into the hold imperceptibly; for no traces of it were visible where it penetrated below the deck; notwithstanding, when no danger was apprehended, the ship was perceived to be on fire forward, about seven hours after she was struck by the lightning. There was stowed in the hold, near the foremast, olibanum, myrrh, and sandlewood: the olibanum, being an inflammatory, resinous substance, was set

The King George, country ship of Bombay, had her foremasts destroyed by severe lightning, and was set on fire.

The lightning set fire to resinous goods stowed near the foremast.

set on fire by the electric matter; and the fire communicated from it to the sandlewood, placed in a large quantity over the olibanum; which had produced a great mass of fire in the hold before it was discovered, when it forced itself through the decks and burnt the ship to the water's edge, although every exertion was made to save her.

The ship was destroyed.

In August 1804, the Bombay frigate, belonging to the Honourable East India Company, at anchor in Malacca Road, was struck by lightning. It embraced the centre masts of the ship and rendered the mainmast unserviceable; although the yards received no injury. The sails were set on fire by the lightning, notwithstanding they were wet by the heavy rain which continued to fall at the time, and assisted the exertions of the crew in extinguishing the fire.

The Bombay frigate had her masts injured, and her sails set on fire; but the yards not damaged.

In July 1804, the ship Page, at anchor in Malacca Road, was struck by lightning. It first embraced the fore-top-gallant-mast head, shivered this mast and the fore-topmast in pieces, and continued its vertical direction down the foremast, tore and rent it greatly, without injuring any of the yards, which were fixed across on these masts. This happened a little after midnight; at which time we were about thirty miles distant from Malacca, and perceived much distant lightning in the direction of it, although none prevailed where we were. On the following night, there was a hard squall from the coast of Sumatra, with much thunder, lightning, and rain. The lightning this night was very vivid, accompanied with a loud hissing noise over the town, but fortunately it did no damage during the night, excepting that the flag-staff of the fort (in the morning) was found injured by it.

Ship Page had her masts greatly injured; but not her yards.

In September 1804, the ship Ardassier, at anchor in Malacca Road, had her main-topmast and top-gallant-mast destroyed by lightning; but none of the yards or caps received injury.

The same befel the Ardassier.

In September 1802, the ship Daniel, distant from Malacca about nine or ten miles, had her fore-topmast and top-gallant-mast destroyed by lightning, during a squall of wind and rain. The topsail and topmast rigging, although wet with rain, were set on fire by the lightning; and, with the topmast, were cut away to save the ship. The yards were not injured by the lightning.

Ship Daniel's fore-topmast, &c. set on fire by lightning; but the yards not affected.

Similar effects of lightning in the ship Trident;

His Majesty's ship Trident, in India, (about 1803), lost her main-topmast and top-gallant-mast by lightning. The yards received no injury.

—the Britannia;

There are at times dangerous lightnings near the Cape of Good Hope. A few years ago, the Honourable East India Company's ship Britannia, returning home from Bengal, was struck by lightning near the Cape. It lodged in the centre of the foremast and set fire to it, whilst lying to in a storm: the violence of the flames was soon so great, that it was found impossible to extinguish them; and the only remedy left to save the ship and crew from inevitable destruction, was to cut away the burning foremast, which was effected; then it fell clear of the ship, over her lee, in a body of fire.

—and the Bombay Castle.

The Honourable East India Company's ship Bombay Castle, about 1801, returning from China, was struck by lightning near Algoa Bay, to the eastward of the Cape of Good Hope. The lightning entered into the head of the foremast, and descending, without making any visible perforation, burst out in fire near the centre of the mast, below the rigging. Every exertion was made to extinguish the fire, without effect: the mast was then cut away, which saved the ship.

General observations.

From the foregoing observations on ships which have been struck by lightning, it may be remarked: 1st.—That it appears always to embrace one of the mast heads at first, and descends downwards. 2d.—That the parts of masts which are covered with tar and blacking, are not so liable to be rent by the lightning as the parts where they are clean scraped, or scraped and covered with tallow. 3d.—That the yards are seldom or never damaged by lightning; although the masts to which they are fixed may be rent into pieces by it. Whether it may be owing to the horizontal position of the yards across the masts, or their being covered with blacking, or a coat of black paint, that is the cause of the lightning not injuring them, when the masts are destroyed, must be left for those skilled in electric phenomena to determine.

V.

Letter from G. S. GIBBES, M. D. *shewing that the Bath Waters contain a much greater Portion of Iron than has hitherto been supposed.*

To Mr. NICHOLSON.

SIR,

I BEG leave to send you an account of some experiments I have lately made on the Bath waters; and as the result appears to me singular, I could wish to know whether any circumstance of the same kind occurs elsewhere.

The sand that rises with the Bath waters contains iron in black particles.

It is well known that many black particles are mixed with the sand which is brought up by the waters of Bath, which particles are attracted by the magnet.

I have lately evaporated large quantities of the Bath waters, and I have obtained a great deal of their solid contents. Upon examining with a microscope the dry residuum thus obtained, I find numberless black particles interspersed through it, and from the circumstance of the magnet acting on those in the sand, I was induced to present the magnet to the black particles of the residuum obtained by evaporation from the water. The result was, that the magnet acted very forcibly on these black particles. By passing the magnet through the powdered residuum, it became charged with these particles of iron; and by brushing them off from the magnet, and again presenting them to its influence, they were again acted on by it, and rose to the magnet from a considerable distance. I repeated the experiment, by evaporating twenty-six gallons of the water of the King's bath, in a brass vessel; and no instrument or vessel was used during the process that was made of or contained the least portion of iron. I obtained 2252 grains of residuum. This residuum was every where interspersed with black particles, and all these particles were very forcibly acted upon by the magnetic influence.

And so likewise does the residuum of the waters.

Iron is deposited in three different states by the Bath waters: 1st.—It tinges the glasses which are made use of for drinking the water at the pumps, of a yellow golden colour, which

The Bath water deposits iron: 1st.—Carbonate.

which can be scraped off. This portion is what, I imagine, was united with carbonic acid, and is deposited on the glasses, on the sides and bottom of the baths, in the state of ochre.

—2d.—Pyrites or sulphuret.

2d.—It forms pyritical incrustations about the reservoirs and channels of the baths; in these the iron is in its metallic state united with sulphur, as they are vitriolizable on exposure to air and moisture.

—3d.—Black sand or metallic.

3d.—It is deposited in the sand of the bath in black particles, which are attracted by the magnet. Some of these particles appeared of a crystalline form, but not sufficiently so to allow me to determine their figure.

The Bath waters contain more iron than has hitherto been supposed.

From the above experiments, which were conducted with much care, it appears that iron exists in the Bath waters in its metallic state, or nearly approaching it. It appears also, that the small portion of iron which is united with carbonic acid, and which is deposited in the baths and in the glasses used for drinking the waters, in the form of yellow carbonate or oxide, is the only portion that has been hitherto estimated as the quantity of iron contained in the waters; which quantity is very inadequate to account for the effects these waters produce on the human constitution.

I am, Sir,

Yours, very respectfully,

Bath, July 4th, 1806. G. S. GIBBES.

VI.

Letter of Inquiry respecting the Cause why Men cannot swim without previous Education as well as Brutes. From a Correspondent, R. B. *With various Observations by the Editor.*

To Mr. NICHOLSON,

SIR,

Remarkable fact that brutes swim naturally, and men do not.

IT has always appeared to me a remarkable fact, that every animal when thrown into deep water should swim without difficulty; but that the human species, if by any accident put into a like situation, is sure to be drowned, unless the individual has been previously educated to keep himself afloat. I have

I have occasionally conversed with many intelligent reasoners on the subject, and must confess that none of the observations I have heard are such as give me satisfaction. By some, it has been remarked, that the superior degree of sensibility of mind in man, compared with brutes, disqualifies him from making the proper exertions in a situation of such novelty and alarm; and others have adverted to the very great difference between the ordinary habits of quadrupeds and men; the former requiring only to perform their usual process of walking when in the water; whereas a man must adopt an unusual position and actions in order to swim. Neither of these remarks appear to me to be well founded. A man of the utmost courage and determination on board a ship, finds those powers of little effect when he is left alone to struggle without skill in the ocean. And though in our artificial method of swimming, (taken from the frog, and very unlike the methods practised by the Asiatics), a man does act very differently from his manner of walking when on the land; yet it will not be pretended that he would sink, if he were to rely on his ordinary walk, as brutes do upon theirs. Upon various former occasions, I have experienced your readiness to discuss such subjects as your correspondents have pointed out. This subject is not, I am sure, unworthy of notice, since the most valuable lives might perhaps be saved, merely by knowledge and information respecting an art which, I strongly suspect, has been thought to require much more practice in its acquirement than it really does, or which, perhaps, may be as natural to us as to other animals.

Questions: Whether man be drowned from terror, or by his usual habits not being adapted to swimming?

Probably from neither.

Importance of this inquiry.

I remain, Sir,

Your constant reader,

R. B.

Observations on Swimming, in Reply to the preceeding Letter.

W. N.

That the practice of swimming is pleasant, healthy, and highly useful, will be admitted by every one. They who cannot swim, are ever ready to deplore their incapacity with all its serious consequences; and those who know their own powers, in that element which so often serves to convey us from place to place, must be highly sensible of the ease and comfort such a consciousness bestows. It is not generally apprehended, that the

Great advantages of the practice of swimming.

It may be learned with very great facility.

the art of swimming may be learned almost at a single trial, and that man is excepted from the general privilege of animals, in this respect, only from a circumstance which it is fully in his power to command; so that if a man, who falls into deep water, can have recollection and resolution enough to avoid one single bad habit (too likely indeed to present itself), he will require no more instructions to enable him to save his life than a dog or any other animal in like circumstances.

A man is no more liable to be drowned than a dog.

Dr. Franklin taught swimming at one single lesson.

I have not at this time in my possession any writing on the art of swimming, except the letter of Dr. Franklin (LIV. in the quarto Exper. and Obser.), which has been deservedly reprinted in many of our periodical works. The Doctor, in the account of his own life, speaks of himself as a first-rate swimmer. He asserts that he taught the art of swimming to a young man of the name of Wygate, and another friend of his, in the course of a few hours; and that he had it in contemplation to establish a swimming school at London, in the summer of 1726. His method, as described in the letter before mentioned, is as follows:

His instructions for becoming familiar with the water.

"The practice I mean is this. Chusing a place where the water "deepens gradually, walk cooly into it till it is up to your "breast; then turn round, your face to the shore, and throw "an egg into the water between you and the shore. It will "sink to the bottom, and be easily seen there, as your water is "clear. It must lie in water so deep as that you cannot reach it "to take it up but by diving for it. To encourage yourself in "order to do this, reflect that your progress will be from deeper "to shallower water, and that at any time you may, by "bringing your legs under you and standing on the bottom, "raise your head far above the water. Then plunge under it "with your eyes open, throwing yourself towards the egg, and "endeavouring, by the action of your hands and feet against the "water, to get forward till within reach of it. In this attempt "you will find, that the water buoys you up against your in"clination; that it is not so easy a thing to sink as you ima"gined; that you cannot, but by active force, get down to the "egg. Thus you feel the power of the water to support you, "and learn to confide in that power; while your endeavours "to overcome it and to reach the egg, teach you the manner "of acting on the water with your feet and hands, which

"action

"action is afterwards used in swimming to support your head
"higher above water, or to go forward through it." page 474.

The Doctor proceeds to make several observations on the natural tendency of the human body to float in the water; at the same time that he advises his friend not to trust to the chance that he may have presence of mind sufficient to take advantage of those hints in a case of real danger, but to learn to swim. The substance of these remarks is: 1st.—That the limbs and head of an human body are heavier than fresh water; but the trunk is lighter, merely because the lungs are inflated. 2d.—That the head is heavier than sea water; but the limbs and trunk are lighter. 3d.—That a person throwing himself on his back in sea water, will float with his face clear, so as to breathe with ease. 4th.—That in fresh water the legs will gradually sink, and the body will float in an erect posture. 5th.—That, in this posture, if the head be held in its natural position, the surface of the water will reach higher than the nostrils, and perhaps a little above the eyes.* 6th.—That if the head be leaned quite back, the nose and mouth will be always above the water and free for breathing, and the body will rise and fall on every such inspiration and expiration.

Dr. Franklin's observations on the natural floatage of the human body.

I am rather surprized at the Doctor's direction about the egg, and the eyes open under water; because it seems as if he thought the submerged experimentalist could see the egg. I know several respectable persons who affirm that they can see objects under water; but as long as we are assured that distinct vision requires that light shoul dcome to a focus on the retina, and that a very considerable part of the refraction performed by the eye is effected at the convex surface of the cornea, intercedent between that substance and the air, we must refuse our credit to such assertions. Besides which, experiment will easily clear up the matter to those who know nothing of optics. For every one may prove by trial, with a bason or tub, that vision under water is scarcely more distinct than that which might be had by looking through a quill or a piece of rough glass.

The Doctor seems to have supposed that man can see under water. This is contrary to optical science,

When I was a boy (in 1770), I had an opportunity of amusing

—and to fact.

* I have always found the nostrils below the surface, and the eyes above it; and I believe there is no great difference in different persons. N.

Instance of an object that could not be seen by the eye, submerged in very clear water.

ing myself by alternately swimming in a deep fresh-water stream, and in the sea, at the Island of Joanna, where both waters are remarkably clear, and are separated by a shingle bank, over which the fresh water runs in a shallow stream. The ground on the sea side consisted of large rolled stones: and as the ship's boat was then on shore taking in water, I assisted in getting the casks out of the fresh water into the sea; for which purpose I had buckled my shoes on my feet. One of my buckles happening to get loose, fell between the stones in about five feet water, where I saw it very clearly. Nothing seemed more easy than to dive and bring it up; and this I did repeatedly, with my eyes open. But I could never see it while my head was under the surface: and though I continued my efforts, and remained obstinately under the water, feeling about for an object, apparently so well within my reach, I was obliged at last to abandon it.

Another instance in Harlem lake, where the eye, under water, could not distinguish the setting sun, nor the fingers of the hand held near the face.

Numberless proofs, that vision cannot be performed by the human eye under water, would have prevented my thinking more on the subject, if the assertions before mentioned had not solicited my attention, and led me to make a direct experiment. Many years after the event at Joanna, I was swimming in the Harlem lake. The sun was almost setting; the water clear; and the bottom a firm sand, gradually deepening from the shore. I walked onwards till the depth exceeded three feet, and then sat down on the sand, where, consequently, I was completely submerged, my face being opposite the setting sun. My eyes were open; the water appeared strongly illuminated, but no image of the sun was to be distinguished. On holding up my hand and spreading the fingers, the hand itself became discernible as an indistinct object, as soon as it was brought within a foot of my face. The fingers could not be counted at half that distance, in any other manner than by reckoning the successive obscurations as the hand was passed across the eyes. All other objects were too confused to be discerned or known. Whence it appears, that all the stories of wonderful divers, who could descend into the sea and bring up small objects, such as jewels and trinkets, must be considered as fabulous.

The stories of divers, &c. not credible.

Dr. Franklin's method of learning to

Dr. Franklin's method of learning to swim, by struggling to descend to the bottom, is better calculated to give courage than skill;

skill; but at the same time it must be allowed, that he who has acquired the former, will require very little of the latter to become a swimmer. I have, nevertheless, remarked, that those boys who were the most daring at plunging into the water before they could swim, have mostly arrived at the art later than others who have attended with some care to the method of striking their arms and legs. I have known several persons who, after acquiring the method of striking the arms separately, so as to have gained confidence to walk in water rising above their shoulders, and of striking the legs while the body was supported by the hands bearing on the ground in shallow water, have swam very well upon the first trial to combine both together.

swim, gives confidence, but not skill.

Method of practising the stroke, &c.

The rules for swimming well, in our method, that is to say, swiftly, and with little fatigue, are few. The body must lie as near the surface, and the head as low as conveniently may be; the knees must be kept wide asunder, in order that the obliquity of action in one leg may counteract that in the other, instead of their joint action producing a libratory motion of the body; and the stroke or impulse must be given with much more velocity than that employed in drawing the legs up again. It is not easy to make any mistake of importance in using the arms by imitation of other swimmers.

Short rules for swimming well.

To swim on the back is so easy, that I have several times taught it at the very first trial, to persons who had no previous notion of supporting themselves in the water. My first care was to assure my pupil that he might depend that his face would continue above water and not sink, when he should be placed in the horizontal position. I directed him to remain in that situation quietly, without the least motion, until he had recovered from any trepidation or confusion of mind, and had acquired confidence from finding that he did not sink. He was then gently to move his hands, in the way of paddling or skulling, which, though not absolutely necessary, would insure the support of his head, and prevent his feet from sinking. And lastly, he was instructed to draw up his legs very gently and strike them out; at the same time bringing his chin towards his breast, to prevent the water from flowing over his face. These instructions, illustrated with appropriate action, being

To swim on the back is extremely easy at first trial. How this is to be done.

first impressed on the mind, I then assisted in supporting him in assuming the horizontal posture. The effects took place as described; the learner found he could swim, and never afterwards forgot it.

Account of a man who had not learned to swim, but who, in a case of extreme danger, supported himself by acting conformably to instructions given him:

After this long, and perhaps garrulous, account of my experience in an art, of which the usefulness and value may afford a sufficient interest to apologize for any prolixity, I must take leave, to introduce another story of a man's life being saved by very simple instructions given him in the moments of extreme danger. The ship Worcester was moored off Culpee, in the Ganges, in November 1770. One of the men, who was employed in some occupation forward about the cables (I believe clearing hawse), slipped into the water, which I am sure was running seven or eight knots (or miles) an hour, as is very common in that river. On the alarm being given, most of those who were upon deck ran aft, where we saw the man's head rise above the water, at the same time that he held up both his hands, and, after a few seconds splashing, sunk again. Soon afterwards he rose a second time, and at that instant the commanding officer, who had a hand trumpet in his hand, called out to him "Keep your hands down in the water." He did so; and remained a considerable time afloat, while one of the boats, which were riding astern, was got along side and manned; and this relief was also retarded by a blunder from too much haste, by which she was cast off without oars on board. His fears must naturally have increased, as his distance from the ship became greater every moment; and I suppose this impression made him again forget his newly acquired art; for he renewed his elevation of hands and dashing of the water, and again sunk; but soon rose again, and for a short time obeyed the incessant and unvaried instruction which was vociferated to him through the trumpet. Whenever he deviated, he sunk; and he had disappeared in this manner at least five times, and had been carried almost out of hearing before the boat took him up; which, however, at last happened, without any injury to his health, as he took an oar and assisted in rowing back to the ship.

—the principal of which instructions was, to keep his hands down in the water.

Men are drowned by raising their arms, the

The particulars of this accident shew clearly how it happens that man is drowned in circumstances where brutes always swim; and why young children are not unfrequently taken up

safe,

safe, after floating a considerable time on the water. In three words, man, by the natural action of raising his hands to save himself by grasping some object, adds to the unfloated weight, and sinks his head. But other animals (a few with small arms and powerful legs excepted) have no notion or ability to grasp, and, in their habitual manner of walking, take the prone posture, which is also very suitable for making progress in swimming. Infants, who have short arms, and little of the habit of seizing and grasping, are less likely to drown themselves than men, because they do not raise their arms.

weight of which depresses the head.

Other animals have no notion or ability to do the same, and therefore swim naturally.

We will conclude by noting the practical results. When a man falls into deep water, he will rise to the surface by floatage, and will continue there if he do not elevate his hands. If he move his hands *under the water* in any manner he pleases, his head will rise so high as to allow him free liberty to breathe. And if he move his legs, as in the action of walking (or rather walking up stairs), his shoulders will rise above the water; so that he may use less exertion with his hands, or apply them to other purposes. By following these plain directions, I have not the least doubt but that any person would swim as readily as the man who fell overboard from the Worcester; and when once the ability to support one's self in the water is acquired, other changes of position and action are performed with facility.

How a man, who knows nothing of swimming, may support himself in deep water.

VII.

Facts towards an History of Gold. By Professor PROUST.

(Concluded from page 247.)

WE are no longer ignorant that in this powder the gold is reduced to the metallic state, because the experiments of Pelletier have demonstrated, that tin, or its oxide at a minimum, applied to the muriate of gold, can never produce any other result. But as it is likewise known, that it contains also a portion of oxide, and this even pretty considerable, it has

Gold in the powder of Cassius in the metallic state.

 generally

generally been supposed, that the purple powder could be nothing but an intimate mixture of gold in the metallic state and oxide of tin.

If, however, we reflect on some properties, which eminently distinguish this purple from a simple mixture of powdered gold and oxide, and in particular the difficulty we find in separating the latter from it, we shall be inclined to suspect, that something more than simple mixture must take place in this precipitate.

Proofs of this.

Let us begin with establishing the metallic state of the gold, after which we will examine the degree of oxidation of the tin that accompanies it.

To analyse the simple powder, we must employ aqua regia, for the nitric or muriatic acid has only a very slow and very imperfect action on it. Scarcely is it wetted with this, before we perceive it lose its colour, yield a solution of gold, and leave the oxide of tin bare. This oxide is heavy, sandy, and transparent as powdered glass, which is the usual charater of the oxide at a maximum. But it may be said, the nitric acid of the solution of gold may have raised it to this degree. I answer, no: for if the purple powder be heated in marine acid, the oxide taken from it is equally vitreous, and its solution no longer precipitates that of gold; it only gives a yellow colour with hydrosulphurated water. There can be no doubt, therefore, that, if the tin accompanying the gold be at a maximum of oxidation, it is because it has taken to itself the oxigen of the gold as it threw it down. Oxide of tin at a maximum, therefore, and gold, are irrefragably the constitutent parts of the purple powder of Cassius.

Component parts of the powder.

Now to begin with showing that this oxide cannot unite with the gold during the precipitation of the purple powder, any farther then a particular affinity attracts it to this metal, I shall here recapitulate some of the properties of the oxide of tin.

The oxide of tin unites with the gold by chemical affinity.

Tin, passing from a minimum of oxidation to a maximum, diminishes in solubility: a fact that has been sufficiently shown in my last paper on tin. In this respect, tin follows the law of most of the metals that are susceptible of two degrees of oxidation. But this decrease of solubility is not the cause of its precipitation in the present instance; for, though less soluble

ble than the oxide at a minimum, it is still very soluble in the muriatic acid, and in aqua regia. For example, let fall a single drop of a very acid muriatic solution at a minimum, into a solution of gold, which is habitually surcharged with it, and you will not fail to produce a purple compound; a purple that certainly has nothing similar to the coloured powders produced by the sulphureous or phosphorous acid, sulphate of iron, &c. Thus, in the case we are examining, there is no reason to suppose, that a few atoms of oxide, which have just acquired a maximum, should thus quit a solvent, that attracts them on all sides, to unite in preference with gold, if the gold did not attract them by a particular affinity. Let me observe too, that the acidity of the liquids increases, in proportion as the gold and tin fall down.

If, therefore, the gold and oxide unite, notwithstanding the obstacle they must have to encounter in such acid menstrua, it is obviously necessary, that a particular attraction should intervene, to save the tin from its customary solubility.

The purple powder perfectly soluble in ammonia.

But a single fact will establish incontrovertibly the particular state of combination, which unites the oxide of tin to the gold. Throw some of the purple powder of Cassius, recently precipitated into a phial full of ammonia, it will dissolve immediately, and impart to the ammonia a vivid and intense purple colour. The solution will pass through the filter, without losing any thing. Water does not decompose it, as it does most of the ammoniacal solutions of metals, unless it be surcharged with the purple powder, and then part of it may separate. Distillation, too, occasions the powder to subside, by carrying off the ammonia; but as long as the liquid contains any ammonia, it continues to hold some of the purple powder in solution. Acids precipitate it from the ammonia in the same manner.

The metallic precipitates of gold are not soluble in ammonia. The oxide of tin at a minimum is but very imperfectly soluble in it, since the solution is always milky. If the purple powder, therefore, dissolve thus copiously and perfectly in ammonia; if it have properties, which neither gold nor the oxide of tin possesses, we must conclude, that these two substances form a real combination with each other: for real combinations alone can have properties strikingly different from those which characterize their component parts. The combination of a metal,

metal, the affinities of which are so limited; with an oxide, the affinities of which are no less injured by extreme oxidation, cannot fail to appear singular; particularly as, I believe, there does not exist any that can be compared with it. It may be objected, that the combination of the oxide of gold with ammonia will not appear, perhaps, less anomalous, if it be closely investigated: for what can be more opposite to the general principles of chemistry, than to see an oxide having a stronger affinity to ammonia than to the most powerful acids? or, than to see in fulminating gold an oxido-ammoniacal combination, which no acid, or even alkali, can overcome?

Mercury cannot take gold from the purple of Cassius.

Mercury, shaken in a phial with fresh purple of Cassius, does not take from it the gold, with which it unites so advantageously on all occasions where there is no combination. If, however, actually metallic gold in the purple powder do not yield to the action of mercury, some other affinity must certainly interpose.

Differs from the precipitate by sulphate of iron:

It has been thought too, that there is no essential difference between gold precipitated by tin, and gold precipitated by sulphate of iron; and even that the latter only required to be mixed or diluted with a white oxide of any kind, to become the purple powder of Cassius. These notions, I must say, have very little foundation: for if gold, precipitated by iron, have something of a purple hue when deposited, it has this tint only in one point of view exclusively; while that of Cassius is constantly purple, in whatever position we look at it.

—and from all other precipitates by combustible substances.

All gold, reduced to the metallic state by any other combustible substance than tin, yields a precipitate, every atom of which refracts the light, so as to make it appear blue when the vessel containing it is placed between the light and eye. Gold, precipitated by sulphate of iron, by phosphorated hidrogen, by sulphureous acid, &c., has this appearance. If we stand between the vessel and the light, we still perceive no purple; but we distinguish the reflection peculiar to each auriferous molecule, in which we clearly recognize particles of metal. But gold, precipitated by tin, on the contrary, is of a deep crimson, is purple, is a velvety powder, which yields no metallic reflection, under whatever angle it is examined, and the tint of which differs from that of its solution in ammonia only by being more intense.

If

If the reader would be still more fully convinced of the real difference, that subsists between the auriferous precipitates and the purple powder of Cassius, it is sufficient to remind him, that, though the muriate of iron acts upon the muriate of gold in the same manner as that of tin, the oxide of iron, which occasions the precipitation, is by no means attracted by the gold like the oxide of tin, though its solubility is as strikingly diminished. If, then, the oxide of iron, raised to its maximum, do not unite with the gold when they come together under the circumstances in which that of tin infallibly does, certainly nothing but the power of affinity can account for the difference. Further proofs of real difference.

Lastly, the purple prepared with tin fixes in silk, and dyes it of a violet colour; which is certainly not the effect of a powder of gold incorporated in the pores of its filaments. Purple of Cassius dyes silk.

In the formation of the purple powder, the excess of the acids has an office very different from what might be supposed; the acids do not take from it any oxide of tin, as it was natural to expect, but they cause, or communicate to the purple powder, a kind of demisolution, which retards its subsidence, and which is on this account very inconvenient, particularly when we are in haste to obtain it. This action of the acids may easily be perceived, if we agitate in marine acid a precipitate recently washed: on doing this, you would say, a solution takes place, and still more if a little heat be employed; but this solution, which imposes on the eye by a kind of transparency, does not stand the test of the filter. Effect of the excess of the acids.

The precipitation of the purple powder may be accelerated by adding potash occasionally to the liquor. If, at the expiration of a few minutes, its surface does not become clear, a little more may be added; and the operator will have the pleasure of seeing the purple powder collecting in clouds and visibly subsiding. In this precipitation, however, an excess is to be apprehended; but this is easily avoided when we are aware of it. The fact is, if more potash than is necessary to neutralize the surplus of acid be employed, it will act on the uncombined muriate of tin held in solution; and thus a portion of oxide will be added to the purple powder, which should make no part of it. Potash hastens the precipitation. —but an excess must be avoided.

Effect

Effect of Acids on the Purple Powder.

Action of aqua regia on the purple of Cassius.

If to a hundred grains of well dried purple powder, a very weak aqua regia be added, such as is composed of marine acid at 4° or 5°, and a few drops of nitric acid, the powder will pretty quickly lose its colour, and afford a solution of gold. This solution, precipitated by sulphate of iron, will yield 24 grains of metallic gold. Only 70 grains of oxide of tin remain, which shows, that the aqua regia has dissolved 6 grains. The oxide is white, and vitreous, like every oxide at a maximum. This result shows us, that the gold, in precipitating, carries with it three times its weight of oxide of tin: and as 76 grains of oxide at a maximum, answer nearly to 72½ of oxide at a mininum, it is probable, that this is the quantity of oxide capable of loading itself with oxigen enough to oxidate 24 grains of gold.

One part gold, to three of oxide of tin.

Further proofs of their chemical union.

If a portion of muriate of tin at a minimum be put into aqua regia, the oxide passes to the maximum state, but is not precipitated, even by the action of heat. When the gold, therefore, in quitting its solvent to be converted into the purple powder, carries down with it such a considerable portion of oxide, this must have resulted from the operation of some affinity.

Muriatic acid takes the tin from the gold.

Muriatic acid of 10°, kept boiling on recent purple powder, decomposes it gradually, and reduces it to the state of pure gold. The power of aggregation connects its particles, and unites them in little lumps, which appear of the colour of gold precipitated by iron, sulphureous acid, &c.

The acid, rendered transparent by subsidence, is a solution of tin at a maximum of a slightly yellow tinge. A slip of tin destroys this colour, but affords no suspicion of purple.

Action of nitric acid.

Nitric acid of 32° takes some tin from the purple powder, and brightens its colour; but never reduces it to the state of pure gold, whatever length of time it be kept boiling. This is the brightening proposed by Lentin.

Lentin's brightening.

The nitric acid, when poured off, holds in solution tin at a maximum, and a little gold. A few drops of muriate demonstrate this in an instant.

This purple, the brightened tint of which approaches to that of cinnabar, still contains tin. Aqua regia detects this easily. It

If it be placed between the eye and the light, we shall perceive, by the blue tint, that the gold begins to mingle with the purple powder.

The aqueous sulphuric acid likewise enlivens the purple, because it takes from it a little tin; but its action goes no farther. Sulphuric acid.

The sulphate of tin at a minimum likewise precipitates gold in the state of the purple powder of Cassius. Sulphate of tin.

Of Gold precipitated by some Vegetable Juices.

I have shown elsewhere, that there are few vegetable juices, whether acids, gummy, saccharine, extractive, &c., which have not the property of disoxidating gold; but among the extractive and colouring juices, there are several which unite with this metal, and form with it purple lakes of a deep and frequently very fine colour. Such unions still farther confirm the disposition gold has to form combinations of a particular order. Gold precipitated by vegetable substances. Purple lakes.

If a solution of gold be poured into a very clear solution of dragon's blood, and the lake be suffered to subside, washed several times in boiling water, and then dried, we shall have a lake compounded of metallic and colouring matter, united by actual combination. With dragon's blood.

If 100 grains of this lake be burnt, and the ashes fused with a little borax, a button of gold, weighing 40 grains, will be produced. A hundred parts of gold, therefore, carry down with them 250 parts, or twice and half its weight of colouring matter. The following facts will show, first, that the gold is in the metallic state in this compound; and secondly, that a combination takes place. 100 parts of metallic gold chemically combined with 250 of colouring principle.

Water is a solvent of the principles contained in dragon's blood: yet water takes nothing from this auriferous lake. Alcohol, which dissolves dragon's blood completely, takes nothing from this lake; it is not even slightly tinged by it. Potash takes from it a great portion of colouring matter, but it does not completely divest it of this; for there still remains a lake of a very fine purple, in which we find the gold united to the colouring principle. Three applications of potash were incapable of reducing the gold to its original purity. Proofs of combination.

 Ammo-

Ammonia takes some of the colouring principle from it, but does not dissolve it.

The muriatic acid at 10° has not the slightest action on this lake, even when it is fresh prepared. The gold in it, therefore, is not oxidated.

Nitric acid attacks it, and is decomposed into nitrous gas, &c.; and the lake is reduced to pure gold by the destruction of the colouring principle.

Lake with pine-bark.

The juice of pine-bark, which is used in Spain for tanning leather, affords likewise a lake similar to the preceding, and having all its properties; but it contains only 25 hundredth parts of gold.

Gold, in the metallic state, therefore, is capable of forming combinations with the colouring parts of vegetables.

On the State of Gold in the Purple applied to Enamels.

Is it metallic gold that colours glass and enamels?

We have seen, that gold is in the metallic state in the purple powder of Cassius; but does it still continue so in the tints it gives to glass and to enamel? This is a question, which, in the present day, it is time to examine, that we may begin at least to throw some light on it, if not to resolve it. Let us take things up a little higher.

Long used for this purpose.

The practice of using gold, either oxided or simply divided, as a purple pigment, is of long standing in Europe. The artists of the seventeenth century employed, indiscriminately, fulminating gold, the oxide precipitated by liquor of flints, gold disoxidated by tin or mercury, gold grated to powder by pumice stone, &c. Homberg and Macquer, afterward, observed, that this metal gave a purple tinge to the vitrified parts of what supported it when it was exposed to the focus of a burning-glass. Ronelle and Darcet, that gold, struck by the electric spark, likewise gave this colour to enamel.

Difference of opinion respecting it.

From the time of Stahl to the discovery of oxidation, chemists were divided in opinion on the nature of the purple. Some thought it too repugnant to the doctrines of chemistry, to admit, that gold, furnished with the whole of its phlogiston, could dissolve in glass, and furnish a solitary instance of a combination, of which there was no example among all the other metals. They concluded, therefore, that it was not capable of colouring glass, but in proportion as it parted with its

its phlogiston; and Macquer could do no less than take this side of the question. It appears, however, observed he, that it is a kind of calcination which renders gold fitted for becoming vitrified. Macquer.

Others, with Orschal at their head, seeing on one hand the facility with which gold simply attenuated produces this colour; and on the other, the low temperature at which its oxides return to the metallic state, concluded, that to divide it well, was sufficient to render it fit for painting. Macquer himself at length adopted this opinion, and is the first who said clearly, "all these facts prove, the purple colour is "natural to gold, whenever it is in a state of extreme division." If it be hereafter discovered, that this doctrine is well founded, let it not be forgotten in France, that to Macquer the honour is to be ascribed.* Orschal. Macquer.

I will recall, to the attention of chemists, some facts, which may guide us to the opinion, apparently the more accurate of the two.

Properties of silver.

Silver is incapable of being oxided by the simple heat of our furnaces; and its extreme readiness to return to the metallic state, when it has been oxided by acids, is another obstacle to overcome. Yet, when an easily vitrifiable substance can dissolve the oxide as fast as it forms, the oxidation, favoured by this attraction, becomes permanent, and supports a heat which is no longer capable of reducing it. The application of leaf silver on glass, its oxide combines with phoshoric or boracic acid, are known proofs of this. The case appears to be the same with gold. If a vitreous surface can dissolve its oxide as fast as it is formed, the reduction is delayed, and the purple maintains itself, till a higher temperature forces it to give up its oxigen.

Oxide of gold may be dissolved in glass.

But the following fact, known in glass-houses, seems to decide the question. Dissolve in soft flint-glass any precipitate of gold, the result will be a brilliant glass, perfectly transpa-

Gold unites with glass, without colouring it.

* To ascribe to Macquer the honour of a discovery, which, according to our author, yet remains to be made, and respecting which he confesses Macquer only adopted the opinion of others, shows Prof. Proust to be more zealous for the honour of France, than for the cause of truth and justice.—T.

 rent

rent and colourless. Will it be said, that the gold is simply divided in this glass? If fragments of it be heated in a retort, and consequently far from any dephlogisticating vapour, they will acquire a superb purple tint, without lossing any thing of their transparency. Can this result be called a metallic reduction? The purple of enamels, and of painting on porcelain, frquently disappears, and afterward reappears with the greatest facility. Do we here recognize a metal that never deviates from metallic simplicity in its different states? gold, dissolved in glass, does, or does not, produce colours. This is a phenomenon in its history of which we know not the cause. Let us then honestly confess with Macquer, "this purple state "of gold is not yet well understood."

—yet this afterward becomes coloured when heated.

This state of gold not understood.

VIII.

Account of an Appearance of Brighton Cliff seen in the Air by Reflection. In a Letter from Dr. BUCHAN.

To Mr. NICHOLSON.

DEAR SIR,

Introduction.

ON reverting to some notes made at the time I found the subsequent account of the appearance, which I mentioned to you in conversation yesterday, and which you seemed to think worthy of recording. Perhaps you, or some of your correspondents, may favour the readers of the Philosophical Journal with an explanation of the optical principles on which the phenomenon depends.

The cliff was seen at sunrise, perfectly represented by an opposite reflection.

Walking on the cliff, about a mile to the eastward of Brighthelmstone, on the morning of the 28th of November 1804, while watching the rising of the sun, I turned my eyes directly towards the sea, just as the solar disk emerged from the surface of the water, and saw the face of the cliff on which I was standing, represented precisely opposite to me at some distance on the ocean. Calling the attention of my companion to this appearance, we soon also discerned our own figures standing on the summit of the apparent opposite cliff, as well as the representation of a windmill near at hand. The reflected images

images were most distinct precisely opposite to where we stood, and the false cliff seemed to fade away, and to draw near to the real one, in proportion as it receded towards the west.

This phenomenon lasted about ten minutes, or till the sun had risen nearly his own diameter above the surface of the ocean. The whole then seemed to be elevated into the air, and successively disappeared, giving an impression very similar to that which is produced by the drawing up of a drop-scene in the theatre. The horizon was cloudy, or perhaps it might with more propriety be said, that the surface of the sea was covered with a dense fog, of many yards in height, and which gradually receded before the rays of the sun. It disappeared as the sun rose.

Perhaps such appearances may frequently occur at sea; a point your own experience may enable you to determine. Is there any analogy between this phenomenon and the celebrated *Fata Morgana*?

I am, Sir,

Your obdient servant,

Percy-street, A. P. BUCHAN.

July 18, 1806.

Remarks. W. N.

The cliff to the eastward of Brighton lies very nearly in the direction S. 72° E. and the sun rose on the 28th Nov. 1804, S. 55° E. The rays of the rising sun, therefore, fell upon the cliff in an angle of about 73° from the perpendicular, on the left hand of the observer who was looking to the southward. Hence the cliff would be considerably illuminated, and the observer not much prevented by the direct solar rays from examining the phenomenon before him. Observations how the cliff bore and the rising sun.

This fact is certainly of great value, as a proof that the effects described under the denomination of *Fata Morgana* (of which an account is given, from Minasi, in the first volume of our quarto series), are not produced merely by light transmitted through the air, or reflected at small angles; but also by direct opposite reflection from some medium suspended in the air. We possess few authentic facts of this last description, and, I believe, no theory at all. Something, probably water, appears to be arranged in the air, perhaps by that slow and regular This fact of high value. Conjectures.

regular deposition that precedes crystallization,* which operates on the light in a manner similar to that of an extended plane surface, disposed nearly in a vertical position; and the rising sun appears to disturb and elevate this mass. But by what laws these causes and their effects may be governed, must be explained by more frequent and minute observation, and, perhaps, from the analogy of experiment.

IX.

Letter from DR. WILKINSON, *respecting the supposed Production of Muriatic Acid from Water by Galvanism.*

To Mr. NICHOLSON.

DEAR SIR,

AT the period Pacchioni's experiments were published, relative to the production of muriatic acid from the galvanic disoxigenation of water, I was engaged in some lectures at Dublin. I employed my extensive apparatus with a view of ascertaining this curious circumstance: my friend Dr. Barker, lecturer on chemistry in that university, provided me with some pure distilled water, the purity of which was ascertained by the usual means with the muriate of barytes and the nitrate of silver: about four ounces of this water was subjected to the influence of 800 plates for thirty hours: about two drams of the water had disappeared: into the residual water, a solution of the nitrate of silver was introduced, the white cloudy precipitation, evincing the existence of muriatic acid, immediately followed. The appearance surprized us much: notwithstanding this, I doubted the accuracy of the experiment, and suspected that muriatic acid had, some how or other, crept into the water. Upon reflection, it appeared to me probably to arise from the acid mixture which is employed in the galvanic arrangements. When at Limerick, I repeated the experi-

Four ounces of water lost two drams by galvanising and the remainder shewed muriatic acid.

In another experiment, performed with extreme caution, no acid was produced.

* Could it have been in frozen plates? Even if so, the Italian phenomenon could hardly have been at so low a temperature.

experiment, with the precaution of having my galvanic apparatus in one room, and my wires, passing through two small holes in a door to the distilled water, in an adjoining room. I employed a similar power as in the former experiment, and decomposed the same quantity: in this there were not the slightest traces of muriatic acid. I am induced to think that such has been the cause of Pacchioni's error. In a periodical work I noticed some experiments of Mr. Peel, of Cambridge. Mr. Peel says he discovered this acid after decomposing two ounces of water. A thousand galvanic plates in troughs, if even replenished every twenty-four hours with fresh acid and water, would not effect the decomposition of so large a quantity of water in three weeks. I wish the particulars of Mr. Peel's experiments, as to the extent of his power, and the nature of the fluid he employs, had been described. Doubts and queries respecting Mr. Peel's experiments.

I am, dear Sir,

Yours, respectfully,

C. WILKINSON.

X.

Letter from H. HAMILT, *jun. Esq. on the Measure of a Ship's Velocity.*

To Mr. NICHOLSON.

SIR,

HAVING lately had occasion to look over the various methods of ascertaining the distance run by a boat, particularly in short distances, such as in surveying harbours, it struck me that the following might answer the purpose.

In the method proposed by M. Pitot, described in Gregory's Mechanics, vol. ii. page 414, two tubes are used, one of which has at its lower extremity, a trumpet mouth, the other straight throughout. Now when the vessel makes way, in his method, her velocity is ascertained by the difference of height to which the water rises in each tube. But as the velocity of a vessel, impelled by oars or sails, especially in a sea not perfectly calm, cannot be perfectly uniform, the water Proposed improvement in Pitot's method of measuring a ship's velocity.

water in the principal tube must have an undulating motion, which will make the velocity difficult to be accurately ascertained.

Now let us suppose the upper orifice of the trumpet-mouth tube to be precisely on a level with the water in the other, and placed in a tub or receiver; when the vessel gets way, the water will rise and flow over, and be caught by the receiver. Now, Sir, I would suppose, that the area of the orifice being exactly found, the space of time in which the water received was flowing in being exactly noted, we might find the velocity of the boat much more accurately (in the above-mentioned circumstances) than by the original method of M. Pitot.

As far as I have read, or inquired, I did not find this method proposed by any one. I now submit it to you, assuring you, Sir, that I am

Your most humble servant,

31, *Dominick-street.* H. HAMILL, Jun.

XI.

Facts relating to the Art of Shaving. From a Correspondent.

HAVING noticed some useful hints in your Journal, on the subject of razors, and their best state of application, I am desirous to contribute one or two observations, to add ease and comfort to their use and operation.

Instructions for shaving, and for preparing the razor.

In the preparation for shaving, I have found it the better mode of applying lather in a thicker state than is usually done, or if laid on thin, to allow it a few seconds to half dry on the face; and, in either case, to rub or brush well into the roots of the hair, by the hand, or by a brush of some hardness, which gives a very useful and moderate resistance to the operation of the razor. In this last article, too, I reverse the common practice, which is to prepare the razor before shaving; on the contrary, I have found it best, from repeated and long experience, to strop it after shaving, and lay it up in that state. It then

then requires only a little rubbing over the palm of the hand, and a dip in warm water, to make it ready for use.

The rationale of this mode I thus explain: The edge of a razor ought to preserve a perfect polish, and this is best done, by letting it lye with a small portion of an oily substance, such as generally is on strops; but which cannot be, if laid by with the vapour or moisture used in shaving, so readily favouring a disposition to contract rust. Rubbing on the hand takes off the small oily matter on the edge, and probably removes a wire edge, common on razors much honed, besides preparing it to receive a small additional warmth and temper from the warm water.

A razor powder; composition not mentioned.

A considerable time ago a razor powder was advertised here, under the title of the "Egyptian Razor Powder."* It is not now publicly sold; but having on its first appearance procured a quantity, I am enabled to speak of it from experience. At first I used it, as directed, in a dry state on the strop; but for a long time past I have mixed it with a small quantity of oil, and found it the best composition for giving an edge to razors, of any I ever tried. For above three years I have never used a hone, nor had a razor ground; and now find a few motions over the strop, after shaving, fully sufficient to preserve my razor, (having never occasion to use a second), in good order.

To some, this may appear a trifling subject; but with many, who, like the addresser, have found a stiff beard and bad razor, very grating attendants on declining years, it will be of some value.

Dublin, July 9th, 1806. H. K.

XII.

Account of the late Eruption of Mount Vesuvius.†

Late eruption of Mount Vesuivius.

ON the 31st of last May, we enjoyed, for the first time, the

* Was this the black powder obtained by triturating tin with a little mercury? I have heard such a powder praised, but have not tried it.—N.

† From the Moniteur of June 22, 1806.

 specta-

Late eruption of Mount Vesuvius.

spectacle of an eruption of Vesuvius. A column of very black smoke rose from the crater about ten o'clock; flashes now and then burst from this column; at length the eruption appeared in a mass of flame, of immense diameter, and occupying the whole vast extent of the crater. This mass was kept up by successive emission of whitish flame, which, as it rose into the air, assumed a more intense red colour. Ignited or melted substances, some of which were opaque, were projected with violence above this body of fire, and often fell beyond the circumference of the crater. At midnight there was not as yet any current of lava, but frequent rumblings were heard.

On the 1st of June, the eruption continued the whole morning, and we resolved to visit the mountain the following night.

We set out at eight in the evening. We took horse at Resina, near the descent to Herculaneum, and proceeded towards the residence of the hermit. The house in which he lives is situated near the southern peak of Mount Somma, being an easy ride of an hour and a half from Resina.

On leaving the hermitage, we proceeded across the valley which separates Somma from Vesuvius, and is known by the appellation of Atrio del Cavallo. It is of no great depth, being almost entirely filled with the lavas of successive eruptions, piled one above another. At length we reached the foot of Vesuvius, where we left our horses, and began to ascend on foot.

The declivity is very steep, and difficult of ascent, on account of the moveable nature of the ground on which you walk, being nothing but a mixture of ashes and fragments of lava, without consistency. After great fatigue we reached the summit, and arrived at one of the edges of the crater.

We had been lighted the whole way by eruptions of the mountain, which were projected to a very great height. Violent rumblings that were continually heard, added to the grandeur and the awfulness of the spectacle, which appeared much more beautiful and majestic from the point to which we had climbed with so much difficulty.

Suspended as it were on the brink of the crater, nothing imposed to prevent our view of the eruptions. We beheld immense masses of flame issuing almost from under our feet, rising above the clouds, and carrying with them, to the same height,

height, showers of ignited stones, which generally descended, nearly in a perpendicular direction, into the very mouth of the crater; but sometimes falling beyond its brink, rebounded around us, and rolled, red hot, down the declivity which we had climbed. Columns of fire, clouds of smoke, and showers of stones, succeeded each other, without interruption, accompanied by continual subterraneous noises; the bowels of the mountain seemed convulsed; the ground on which we stood shook, and threatened to sink beneath our feet. Never had we beheld a more melancholy image of the convulsions of nature, and notwithstanding the risk we incurred from the continual falling of the stones, we could scarcely be prevailed upon to leave it.

Late eruption of Mount Vesuvius.

Our guides, who were better judges of the danger than ourselves, now became alarmed, and urged us to descend.

The violence of the volcano had increased since we reached the summit, and the Power that presides over the place, seemed inclined to punish us for our audacity, and for having presumed to violate his tremendous abode.

We accordingly descended, and in a few minutes arrived at the Atrio del Cavallo. We were out of the reach of danger, and were enabled to contemplate, without apprehension, the objects by which we were surrounded. What an admirable spectacle! Over our heads, the volcano, with its smoking lava rushing down the sides of the mountain; before us, the sea smooth and calm; the full moon illumining with her mild beams the extremity of the horizon; the clouds and the smoke, wafted around the summit of the mountain, and concealing, for a few moments, the vast conflagration, which appeared again more lively and more brilliant; this succession of lights and shades, this contrast of turbulence and tranquillity, this solitude in the midst of such a vast convulsion, produced a multitude of contrary impressions, that cannot be described, but the recollection of which will never be erased.

We returned about four in the morning to Naples, having spent eight hours in the excursion.

On the second, the eruption continued the whole day with much greater violence than before; two currents of lava were formed; one of these stopped in the morning; the other, taking an eastern course, spread with great rapidity, and de-

Late eruption of Mount Vesuvius.

luged the plain. As our excursion of the preceding night had not enabled us to form any idea concerning the progress of the lava, we set out again to observe this extraordinary phœnomenon.

Passing through the villages of Portici, Resina, and Torre del Greco, we entered inclosures, consisting of vineyards and corn-fields, into which the lava had penetrated ; we approached the current, and I was surprized to find the progress of the lava so different from the conception I had formed of it.

I had always imagined that the substance of the lava, resembling melted glass, ran in the same manner, and advanced uniformly like a river of fire ; and indeed it is extremely probable that in a great number of eruptions it actually exhibits this appearance. On the present occasion, I saw nothing but an accumulation of stones, some of which were of prodigious magnitude, heaped one upon another to the height of fifteen or twenty feet, and about half a mile in breadth. This formidable mass advanced slowly, following a progression produced solely by the falling of the different bodies, between which there was no adhesion, and which, in obedience to the impulsion they had received on issuing from the crater, rolled from the most elevated point and covered the surface of another *stratum.* In this manner the stones rolled over one upon another, till the front rank having attained the same height as that which produced it, began in its turn to pour down the ignited bodies that came tumbling upon it.

All this intestine motion was accompanied with a noise, resembling the decrepitation of salts, but much more loud and brisk. The fire was fed by various combustible matters, as sulphur, bitumen, and metals, which might be known by their flames ; but there was no appearance either of complete fusion, or of the commencement of it. The stones resisted the the pressure of a stick, which I several times endeavoured, but in vain, to thrust into them.

Meanwhile the devastation occasioned by the progress of this torrent, presented a horrid spectacle. The trees which supported the vines, and the vines themselves, were burned by the extreme heat of this mass of matter, even before it reached them ; and the bright and clear light produced by their combustion, indicated the exact *contour* of the progress of the lava.

The

The walls of inclosures and of houses, calcined by the heat, crumbled to pieces before this moving mountain, or were thrown down by the force of the impulsion. Sometimes, however, instead of overturning an obstacle, the lava turned aside, and left it standing; for this variety of action it is impossible to assign any reason.

Late eruption of Mount Vesuvius.

After we had contemplated this dismal and astonishing sight, we went up to the convent of the Camaldulenses, situated on a kind of peak, of considerable height, which overlooks the whole plain, that extends from the south to the west, from the foot of Mount Vesuvius to the sea. This building has hitherto been spared, as well as the thick wood in which it is embosomed. It is one of the nearest points to Vesuvius, and that from which you are best able to discover and trace the progress of the lava. It is the asylum to which the wretched inhabitants of the desolated plain have often fled with their most valuable effects; to which they have driven their flocks, and conveyed their wives and children.

Here we staid a considerable time: our view extended over the declivity of Vesuvius, from which ran several currents of lava, that issued from the sides of the mountain; while enormous flames of fire, of which we had a nearer prospect the night before, darted continually from its summit. We had likewise a view of the plain, in which appeared the long windings of the rivers of fire. The reddish reverberation of the lava, and the conflagration in the plain, illuminated the landscape. On every side appeared the image of desolation: but yet it exhibited a picture so splendid, a scene so magnificent, that the ravages with which it was attended, were entirely forgotten in the contemplation of its picturesque and poetic beauty. In short, when my mind figures to itself those fiery torrents, the motion of the lava, the subterraneous thunders, those continual hissings, so many wonders, so many subjects of grief and admiration, I should think that a dream had deceived me, if the imagination, which produces such dreams, were capable of creating images so awful and so grand.

On the 3d, the eruption continued, and the lava still advanced; the detonations were louder, and more frequent, than the preceding day. In the evening, the flames shot to a still greater

greater height, attracting the electricity of the air and of the clouds, which emitted splendid flashes.

On the 4th, the eruption was less violent.

On the 5th, Vesuvius began to throw out ashes, which, we are assured, announces the conclusion of the eruption.

XIII.

SCIENTIFIC NEWS.

Galvanic Society of France.

The Electro-micrometer of de Launay,

ON the 6th of Febuary last, Messrs. Nauche and Tourlet gave an account of the Transactions of the Society in the years xii and xiii. The electro-micrometer of Veau de Launay was described by Marechaux as an instrument proper for shewing the smallest perceptible quantities of electricity and of galvanism, and capable of being applied to many useful atmospherical researches.

—is an old invention.

This electrometer does not differ from one which I constructed many years ago, and of which I published an account in the quarto series of this Journal, in thenumber for September 1797. It consists in the application of two metallic peices, which, by means of a screw, are made to approach on each side of the points of the gold leaf in Bennett's electrometer. The Professor D. L. published his instrument in the Journal de Physique for July 1804. When we consider the slowness and difficulty of communications, and the simplicity of the contrivance, there will be no ground for detracting from the merits of the latter inventor.

Craniologic System of Gall.

Craniology.

Mr. Klauer, a sculptor of Weimar, has executed busts of Schiller and Weiland, in plaster. The mask of Schiller was modelled on his face after death, by Professor Ingram. Weiland, during the stay of Dr. Gall at Weimar, had the com-

complaisance to permit his head to be modelled entire. Craniology. The writer of this article adds, that if craniology should continue to be fashionable in Germany, it will be but a point of common prudence in all celebrated men to follow this example, in order that their craniums may be suffered to remain in their graves. The same artist, Klauer, has executed in plaster a cranium, furnished with all the protuberances and organs for the use of students of the theory of Dr. Gall.

We also learn, that Dr. Kelch, professor of anatomy, at Konigsberg, has examined the cranium of the celebrated Kant, and finds that it is amply provided with all the protuberances and indentations which Dr. Gall has announced as indicative of the talents which that German philosopher has displayed.

Greek Manuscripts probably existing in Russia.

Probability that many Greek MS. may be recovered in Russia.

The St. Petersburgische Monathschrift, a monthly Journal, published in the capital of the Russian empire, contains an article which is highly interesting to literature. It relates to the progress of knowledge and civilization in Russia, from the earliest ages to the time of Peter the Great. The most striking part, is that in which considerable hopes are entertained that some of the Greek manuscripts, which are supposed to be lost, will be recovered in that empire. Kotzebue, in his periodical work, gives the following abridged account.

Narrative respecting them.

Jaroslas I. son of the great Waladimir, caused a great number of learned men to come from Greece, and employed them to translate into Sclavonian those Greek books of which the originals were at Kiow, in the church of St. Sophia. Constantine was so much attached to the sciences, that he possessed more than a thousand Greek manuscripts, many of which were translated by his orders into Sclavonian, and distributed in the schools. Alexis Michalowitz being desirous of comparing the translations with the text, purchased in Greece, particularly at Mount Athos, about five hundred manuscripts, which are still at Moscow, in the library of the Synod. If it be allowed that this last collection may consist almost totally of bibles, or the works of the fathers, it may nevertheless be

Greek manuscripts.

be asked, with Mr. Kotzebue, whether the thousand manuscripts collected by Constantine be of the same description? what may have become of those which he presented to the schools? and whether the still more numerous collection of Jaroslas I. be not still in the church of St. Sophia? The desire expressed by this learned journalist, that all convents in Russia might be ordered to give catalogues of their libraries, will be sincerely adopted by all the lovers of literature. It is more than probable, that some very valuable remains of Greek literature would be by this means restored.

TO CORRESPONDENTS.

To correspondents.

From the great accession of Original Correspondence, it becomes necessary to take notice of several Communications that have not been immediately inserted; most of which will appear in the next and the Supplementary Number. They are,

1. *Account of the Treatment of Lead Ores; with Drawings of Furnaces: By Mr. John Sadler.*—2. *A Dissertation on the Claims of Robert Hooke, Harrison, and others, to some of the principal Inventions in Chronometry: By Mr. Thomas Read, of Edinburgh.*—3. *New Experiments on the Propagation of Heat in Liquids; with a Drawing: By Benjamin Count of Rumford.*—4. *Experiments and Observations on the Adhesions of the Particles of Water to each other: By Benjamin Count of Rumford.*—5. *Remarks on Achromatic Eye-pieces: By David Brewster, A. M. N. B. This gentleman informs me that he has lately invented a new Telescope, capable of measuring distances from two Stations, with the greatest facility and accuracy.*—6. *A Method of ventilating Tents: By Lieutenant William Collins.*—7. *Two Communications from H. B. K. on Galvanic and Chemical Subjects.*—8. *Some Speculations from a Correspondent, L. upon certain Anomalies in the Specific Gravities of Water and other Bodies.*—9. *A new Method of rendering the wide and narrow Vibrations of an Helical Spring equal in Duration: By Mr. Hardy.*—10. *An Improvement in the inflammable Air Lamp of Volta: By Mr. Singer.*

Invention of N Chronometers

Fig. 1

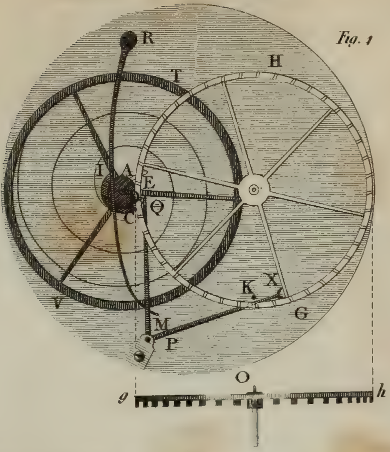


Fig 1

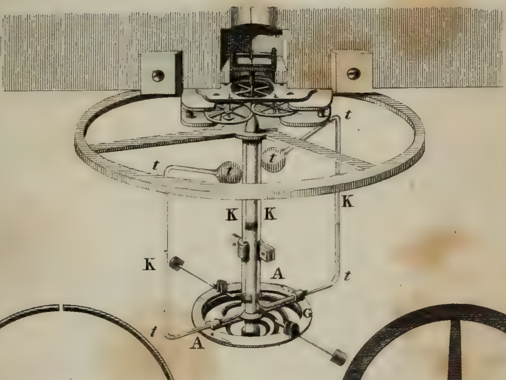


Fig 2

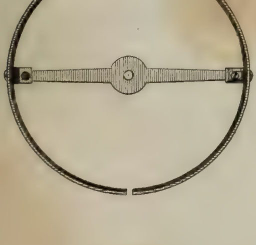
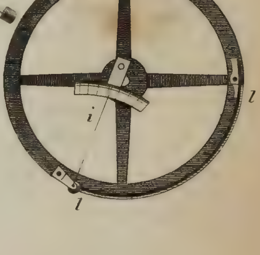
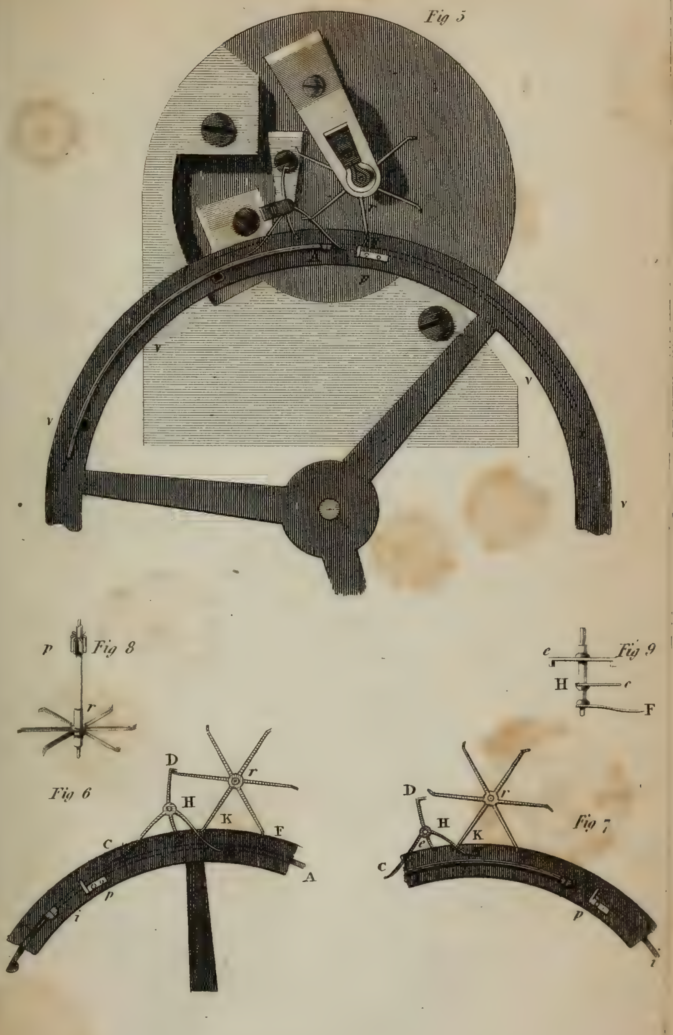


Fig 3



Invention of Chronometers



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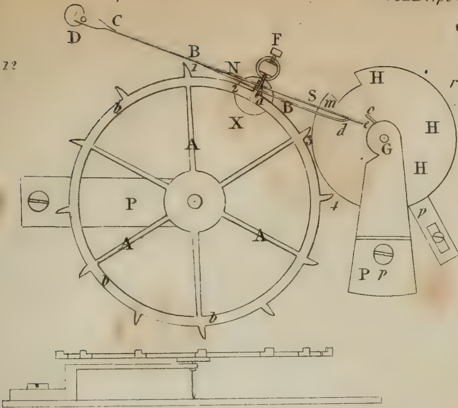
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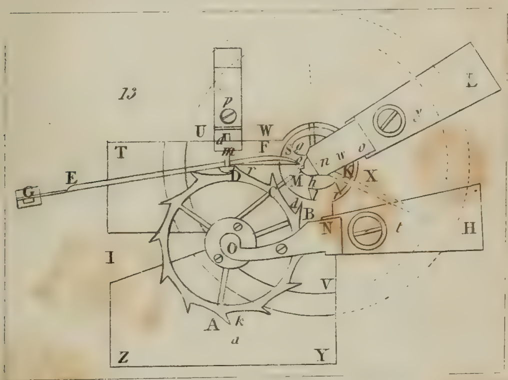
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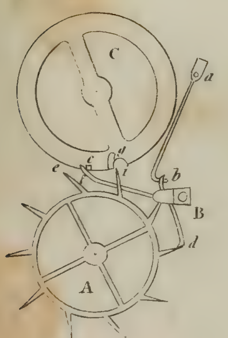
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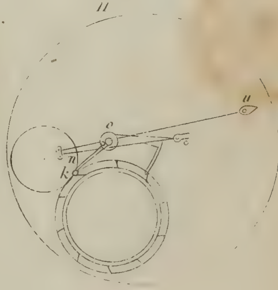
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JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1806.

ARTICLE I.

Inquiries concerning the Mode of the Propagation of Heat in Liquids. By BENJAMIN COUNT OF RUMFORD, *F.R.S. Read at the National Institute of France, 9th June*, 1806. *Translated by* W. CADELL.

THE motions in fluids which result from a change in their temperature, give rise to so great a number of phenomena, that philosophers cannot bestow too much pains in investigating that interesting branch of knowledge. Motion of fluids by heat.

When heat is propagated in solid bodies, it passes from particle to particle, *de proche en proche*, and apparently with the same celerity in every direction; but it is certain that heat is not transmitted in the same manner in fluids. Passage of heat through solids.

When a solid body is heated and plunged in a cold liquid, the particles of the liquid in contact with the body being rarefied by the heat that they receive from it, and being rendered specifically lighter than the surrounding particles, are forced to give place to these last and to rise to the surface of the liquid; and the cold particles that replace them at the surface of the hot body, being in their turn heated, rarefied, and forced up; all the particles thus heated by a successive contact with the hot An heated solid does not heat a liquid downwards.

body form a continued ascending current, which carries the whole of the heat immediately towards the surface of the liquid; so that the strata of the liquid situated at a small distance under the hot body are not sensibly heated by it.

A cold body cools a liquid downwards and not upwards.

When a solid body is plunged in a liquid which is hotter than the body, the particles of the liquid in contact with the body being condensed by the cooling they undergo, descend in consequence of the increase of their specific gravity, and fall to the bottom of the liquid; and the strata situated above the level of the cold body are not cooled by it immediately.

The viscidity of fluids prevents their particles from moving singly.

It is true, that the viscosity of liquids, even of those which possess the highest known degree of fluidity, is still much too great to allow one of their particles individually being moved out of its place by any change of specific gravity occasioned by heat or cold; yet this does not prevent currents from being formed in the manner above described, by small masses of the liquid composed of a great number of such particles.

Currents in fluids.

The existence of currents in the ordinary cases of the heating and cooling of liquids, cannot any longer be called in question; but philosophers are not yet agreed with respect to the extent of the effects produced by those currents.

Conductors and non-conductors.

In treating of abstruse subjects, it is indispensably necessary to fix with precision the exact meaning of the words we employ. The distinction established between *conductors and non-conductors* of heat is too vague not to stand in need of explanation. An example will shew the ambiguity of these expressions.

Instance of heat transmitted

If two equal cubes of any solid matter, copper for instance, of two inches in diameter, the one at the temperature 60°, the other at 100°, be placed one above the other; the cold cube will be heated by the hot one, and this last will be cooled.

—through a metallic plate;

If the cold cube be placed upon a table, and its upper surface covered by a large plate of metal, of silver for instance, a quarter of an inch thick, and if the hot cube be placed upon this plate, immediately above the cold cube, the heat will descend through the metallic plate with a certain degree of facility, and will heat the cold cube.

—more slowly through a board,

If a dry board, of the same thickness with the metallic plate, be substituted in its place, the heat will descend through the wood, but with much less celerity than through the plate of silver.

But

But if a stratum of water, or of any other liquid, be substituted in place of the metallic plate, or of the board, the result will be very different. If, for instance, the cold cube being placed in a large tub, resting on the middle of its bottom, the hot cube be suspended over it by cords, or in any other manner so that the lower surface of the hot cube be immediately above the upper surface of the cold cube, at the distance of a quarter of an inch, and the tub be then filled with water at the same temperature as that of the cold cube, the heat will not descend from the hot cube to the cold one through the stratum of water of a quarter of an inch in thickness that separates them.

—and not downwards through water.

We may with propriety call silver a *good conductor* of heat, and dry wood a *bad conductor;* but what shall we say of water? I have called it a *non-conductor*, for want of a more suitable term; but I always felt that that word expresses but imperfectly the quality that was meant to be designated.

Silver is a good conductor; wood a bad one; and water a non-conductor.

In the experiment of the two cubes plunged in water, if the hot cube be placed below and the cold cube above it, the heat will not only be communicated from the hot to the cold cube, but it will pass even more rapidly than when the two cubes are separated by a plate of silver. But in this case it is evident that the heat is *transported* by the ascending currents, which are formed in the liquid in consequence of the heat which it receives from the hot body.

Water may *transport* heat by currents.

The existence of these currents, in certain cases, has been known a long time; but philosophers have not been sufficiently attentive to the many curious phenomena that depend upon them. It has not even been suspected with what extreme slowness heat passes in fluids, from particle to particle, *de proche en proche*, in cases where the effects of such communication become sensible.

Direct conduct of heat through water, extremely slow.

For some time after I had engaged in this interesting inquiry, I conceived that this kind of communication was absolutely impossible in all cases; but a more attentive examination of the phenomena has convinced me that this conclusion was too hasty. As early as the beginning of 1800, in a note published in the third edition of my Seventh Essay, I announced a conjecture that the non-conducting power of fluids might perhaps depend solely on the extreme mobility of their particles;

Liquids probably *conduct* only when their particles are not allowed to move.

cles; and it is certain, if this conjecture is founded, liquids must necessarily become conductors of heat (though very imperfect ones) in all cases where this mobility of their particles is destroyed, as well as in these rare but yet possible cases, where a change of temperature can take place in a liquid without giving its particles any tendency to move, or to be moved out of their places.

In all ordinary cases they *transport* heat.

The unequivocal results of a great many experiments have shewn, that in ordinary cases, and perhaps in all cases where heat is propagated in considerable masses of fluids, its distribution is accomplished precisely in the manner that the new theory supposes, that is to say, by currents. And it is certain that the knowledge of that fact has enabled us to explain in a satisfactory manner several interesting phenomena of nature, which before were enveloped in much obscurity.

It is known that an hot body heats a fluid upwards; but can it do this downwards?

When a hot solid body is plunged in a cold liquid, there can be no doubt concerning the existence of the vertical ascending currents which are formed in the liquid, and which convey to the surface the heat which its particles have received; but with respect to the strata of liquid situated under the hot body, *are they, or are they not heated by this body, by means of a direct communication of heat from above downwards, from particle to particle, these particles remaining in their places?* This is a question on which philosophers are not yet agreed. As it is a question of great importance, I have long meditated on the means of deciding it; and after several unsuccessful attempts, I have at last succeeded in making an experiment which I think is decisive.

New experiment.

As the apparatus which I used for this experiment, and which I have the honour of laying before the assembly, is somewhat complicated; and as it is indispensably necessary to be intimately acquainted with it, in order to form a judgment concerning the degree of confidence which the results of the experiment may deserve, it is necessary to give a detailed description of this machinery. The annexed figure gives a distinct representation of its principal parts. It is drawn on a scale of a quarter of an inch to the inch, English measure.

A B, Fig. 2, Plate X. is a board, of oak, seen in profile; it is $1\frac{1}{2}$ inch thick, 18 inches long, and 11 inches in breadth. It serves to support two square upright pillars, C C, $18\frac{1}{4}$ inches

in

in height, and 1½ inch square. They are firmly fixed in the board, at the distance of 11 inches asunder, and serve to support the two cross-pieces, D E, F G, at different heights.

Apparatus for experiments in fluids.

These cross-pieces are each pierced with two square holes, at the distance of 11 inches one from the other, into which the upright pillars, C C, enter, and the cross-pieces are supported at any height that is required, by means of a screw of compression. These screws are represented in the figure.

The cross-piece, F G, which is represented in profile, is 17 inches in length, and 1½ inch thick, and 3 inches in breadth. It is pierced in the middle by a cylindrical hole of 2 inches in diameter.

The cross-piece, D E, is 17 inches in length, by 1½ inch in thickness. It is 3 inches wide at each end, and 6 inches in the middle, where it is pierced by a circular hole 5 inches in diameter.

The cross-piece, D E, serves to support the annular vessel, H I, of which a vertical section, passing through its axis, is seen in the figure. This vessel, formed of thin brass plates, is 5 inches in diameter without, 3 inches in diameter within, and 27⅛ inches in depth. This vessel is filled with water during the experiments to the height of 2½ inches; and its form is such, that if the water that it contains were frozen into a solid mass of ice, this piece of ice would have the form of a tube, or perforated cylinder, of one inch in thickness and 2½ inches high, by 5 inches in diameter without. Its cylindrical cavity would be precisely 3 inches in diameter.

K L is a vertical and central section of a cylindrical vessel of tin, of 10 inches in diameter, by 4½ inches in depth. It is filled with water to the height of four inches, as it is seen in the figure.

The cross-piece, D E, is placed at such a height that the bottom of the annular vessel, H I, is plunged a quarter of an inch under the surface of the water contained in the great cylindrical veesel, K L.

In the axis of this last vessel is placed a small hemispherical cup of wood 2 inches in diameter without, and half an inch thick. It is kept in its place by a short vertical tube of tin, soldered to the bottom of the cylindrical vessel, K L, into which the stalk of the cup fits tightly.

The

Apparatus for experiments in fluids.

The middle of the cavity of this cup is occupied by the bulb of a small mercurial thermometer of great sensibility. Its tube, which has an ivory scale, is laid down horizontally, and fixed in one side of the cup, through which the tube passes, in such a manner that the lowest part of the bulb is elevated $\frac{1}{10}$ of an inch above the bottom of the cup. The diameter of the bulb being $\frac{3}{10}$ of an inch, and the hemispherical cup having $\frac{1}{2}$ inch of radius within, it is evident that the upper part of the bulb is $\frac{1}{10}$ of an inch below the level of the brim of the cup that contains it. To avoid charging the figure with too many details, the scale of the thermometer is not drawn, but the tube is distinctly represented.

The horizontal cross-piece, F G, serves to support a very essential part of the apparatus which remains to be described.

This cross-piece supports, in the first place, a vertical tube of wood, M, 6 $\frac{6}{10}$ inches in length, and 2 inches in diameter without. Its interior diameter is $1\frac{1}{20}$ inch. This tube is supported by a projecting collar (represented in the figure) $2\frac{1}{2}$ inches in diameter, which rests on the cross-piece, F G. It is a vertical and central section of this tube that is represented in the figure; and it is dotted, in order to distinguish it from the surrounding parts of the apparatus.

The lower part of this tube is plunged $\frac{6}{10}$ of an inch under the surface of the water in the large cylindrical vessel, K L; and it is placed precisely above the wooden cup in the prolongation of its axis, the lower extremity of the tube being at the distance of $\frac{3}{10}$ of an inch above the horizontal level of the brim of the cup.

On the top of the tube of wood is placed a cylindrical vessel, N O, of sheet brass, 3 inches in diameter, $2\frac{3}{4}$ inches high; which has a lateral spout, P Q, placed a little above the level of its bottom.

From the middle of the bottom of this vessel there descends a cylindrical tube of brass, 6 inches in length, and 1 inch in diameter, which ends below in a hollow conical point, as represented in the figure.

R S is a vertical and central section of a funnel of brass, which ends below in a cylindrical tube of $\frac{3}{10}$ of an inch in diameter, and 6 $\frac{6}{10}$ inches long. This funnel is kept in its place, in the axis of the cylindrical vessel, N O, by the exact fitting of its upper edge upon that of the vessel into which it is adjusted.

The

The lower end of the tube of this funnel is surrounded by a projecting edge, or flanch, in the form of a hollow inverted cone. The diameter of this conical projecting brim above, at its base, is $\frac{7}{10}$ of an inch, and it is soldered below to the end of the tube.

Apparatus for experiments in fluids.

When hot water is poured into the funnel, this liquid, descending by the tube of the funnel, strikes against the inner surface of the hollow inverted cone, which terminates the vertical tube that belongs to the vessel, N O, and then rising up through this last tube into that vessel, it runs off by its spout. It was with a view to force this water to come into more intimate contact with the hollow cone, that the projecting edge, in form of an inverted cone, was added to the lower end of the tube of the funnel.

The object chiefly in view in the arrangement of this apparatus, was to give to the conical point which terminates the vertical tube of the vessel, N O, an elevated temperature, which should remain constant during some time, for the purpose of observing if the heat, which must necessarily be communicated by this metallic point to the small quantity of water with which it is in contact, and which is confined in the lower part of the wooden tube, M, would descend, or not, to the thermometer which was placed in the wooden cup.

There was still one source of error and incertitude against which it was necessary to guard. The heat communicated through the sides of the wooden tube to the water contained in the great cylindrical vessel, K L, might be transported to the sides of that vessel, and being then communicated from above downwards through these sides, might heat successively the lower strata of the liquid, and at last that stratum in which the thermometer was.

It was to prevent this, that the annular vessel, H I, was used; and it performed its office in the following manner.

The particles of water contained in the great vessel, K L, which, being in contact with the exterior surface of the wooden tube, were heated by that tube, could not fail to rise to the surface, and there they necessarily came into contact with the interior sides of the annular vessel to which they communicated the excess of heat they had received from the wooden tube.

This

This heat, passing readily through the thin metallic sides of that vessel, was given off as fast as it was received to the particles of cold water contained in the vessel which were in contact with its sides, and these particles rising to the surface of the water contained in the annular vessel in consequence of their acquired heat and levity, the progress of the heat from the wooden tube to the sides of the large vessel, KL, was interrupted; and all the heat that passed through the sides of the wooden tube was by these means turned aside in such a manner that it could no longer disturb the progress of the experiment, nor affect the certainty of its results.

The apparatus and experiment. A conical metallic piece pointing downwards in water, was kept hot. It gave heat to a small portion of water confined by a wooden cylinder, open beneath to a larger vessel. Provision was made to prevent heat being carried down by the sides of the vessel. A thermometer at a small distance below was not affected.

Before I proceed to give an account of the result of this inquiry, I shall take the liberty to recall the attention of the assembly to the most important circumstances of the experiment.

On pouring boiling water in a small uninterrupted stream into the funnel, the hollow conical point which terminates the vertical tube belonging to the vessel, NO, was heated, and kept at a constant temperature little under that of boiling water.

This point was surrounded by a small quantity of water contained in the cavity of the lower part of the wooden tube, and as this water could not change its place nor be displaced by the surrounding cold water, being enclosed and protected by the sides of the wooden tube, it would necessarily become very hot in a short time.

But this small quantity of hot water lay immediately upon a stratum of cold water, which separated it from the bulb of the thermometer, placed directly under it at the distance of only half an inch.

If heat could pass in the water from above downwards, it would no doubt pass from the lower stratum of hot water, contained in the open end of the wooden tube to the bulb of the thermometer, which lay immediately below it, and at so small a distance.

Three experiments were made with this apparatus, and always with exactly the same results. In the first, a stream of boiling water was poured into the funnel during 10 minutes. In the second, during 12 minutes; and in the third, during 15 minutes.

The

The thermometer, whose bulb was in the wooden cup, remained *at perfect rest* from the beginning of the experiment to the end of it, without shewing the slightest sign of being in any way affected by the hot water which was so near it.

These experiments were made at Munich, in the month of July 1805, the temperature of the air and of the water contained in the vessel, KL, being 70° Farenh.

A small thermometer, placed in the water, contained in the annular vessel, H I, in such a manner that its bulb was scarcely submerged, marked that this water had received a little heat in each of the three experiments.

Another similar thermometer, placed in the water contained in the large vessel, KL, immediately under its surface and near one side of the vessel, shewed that this water had not acquired any sensible increase of temperature during the experiments.

From the results of these experiments we are authorized to conclude, that heat does not descend in water to a sensible distance, in cases where the particles of the liquid which receive heat are exposed to be displaced and forced upwards by the surrounding colder and denser particles; that is to say, in all the cases (and they are the most common) where heat is applied to the strata of the liquid situated under its surface.

The experiment shew that heat does not pass downwards in fluids free to circulate.

But the results of the experiments in question do not prove that heat cannot in any case descend in water; and still less can it be inferred from them, that all direct communication of heat, in this liquid, from particle to particle, *de proche en proche*, is impossible. They do not even prove that heat did not descend, *to a small distance*, below the level of the end of the wooden tube, in these experiments; for it is certain that that event could take place without the thermometer, which was situated a little lower, being in any way affected by that heat.

But they do not prove that fluids cannot conduct directly.

The particles of water, situated at a very small distance below the level of the lower end of the wooden tube, being heated by the stratum of hot water which rested immediately on them, might have been displaced by the surrounding colder and denser particles, and forced to rise to the surface; and these last being in their turn heated, forced upwards and replaced by other cold particles, it is evident that the heat could not make its way downwards so far as to arrive at the thermo-

The fluid above the thermometer was not quiescent.

meter through a stratum of liquid, which though apparently at rest, was nevertheless in part composed of particles which were continually changing.

Opinion of the author that the imperfect conducting power of fluids is owing to the mobility of their parts.

I have long suspected that the apparent impossibility of a direct communication of heat between neighbouring particles of fluids depends solely on the great mobility of those particles (see a note, p. 202, tom. ii. of my Essays, third edition, London, 1800); and if this suspicion be well founded, it is certain that when this mobility ceases, the effect which depends on it must cease likewise.

Not of their particles; but of sensible portions or parts.

When I speak of the mobility of the particles of a liquid amongst each other, I am very far (as I have already observed) from supposing that individually they can enjoy a free motion. I was formerly of that opinion, but a more attentive investigation of the phenomena has convinced me that I was mistaken. But although one individual particle of a liquid can never be put in motion in consequence of a change of its specific gravity occasioned by a change of temperature, yet what cannot happen to a single particle, may easily, and must necessarily, happen to small masses of the liquid consisting of a great number of these particles united; as is abundantly proved by the currents which are so easily excited by the contact of a hot or cold body plunged in a liquid.

The particles of fluids have much adhesion.

The force by which the particles of liquids adhere together is very great; and it is more than probable that it is the cause of many very interesting phenomena, and amongst others, of the suspension of the heavy bodies which much lighter liquids so frequently hold in solution.

Estimate of its quantity.

From the result of an experiment which I made some years ago, in order to determine the measure of the viscosity, or the want of perfect fluidity in water, at the temperature of 64° Farenh. I found reason to conclude, that a solid body having a surface equal to 368 square inches, which should weigh only one grain troy more than an equal volume of water, would remain suspended in that liquid; and from this datum it is easy to find by calculation, what ought to be the diameter of a small solid spherule of the heaviest matter, of gold for instance, in order to its remaining suspended in water in consequence of the viscosity of that liquid.

Having

Spherule of gold which would not work in water.

Having made this calculation, in order to satisfy my curiosity, I found that a solid spherule of pure gold, of the diameter of $\frac{1}{300,000}$ (or exactly $\frac{1}{298719}$ of an inch), ought to remain suspended in water in consequence of the adhesion of the particles of that liquid to each other. But I shall return to this subject on a future occasion.

II.

An Account of the Invention of the Balance Spring, and the Determination of the Conditions of its Isochronism in wide and narrow Vibrations, by Robert Hooke, in the 16th Century, and of the first free Escapement by Du Tertre; together with various other Historical Details relative to Timepieces. In a Letter from Mr. THOMAS REID *of Edinburgh.*

To Mr. NICHOLSON.

SIR,

Reference to Arnold and Earnshaw's timepieces.

IN your Journals of late, I see you have given an account of the detached 'scapement, such as it is now generally applied to chronometers or timekeepers, and also of the different forms, and the properties of the pendulum spring, both as explained by Messrs. Arnold and Earnshaw to the Honourable Board of Longitude.

Robert Hooke, the inventor of the pendulum spring unjustly neglected.

I beg you will allow me, through the channel of your interesting and useful Journal, to give you some account of the invention of the pendulum spring and of its properties, by their author, our countryman, the celebrated Dr. Hooke. I have been often provoked to see his name so much kept in the background in regard to these matters, and particularly by foreign artists, who, whenever they have occasion to speak or make mention of the pendulum spring, enter much on the merits of this important invention, and are full of enthusiasm in praise of M. Huyghens, for having made this wonderful discovery. M. Huyghens was undoubtedly one of the most profound geometricians that any age has produced, and Dr. Hooke must certainly be allowed to have been one of the greatest mechanicians; a bare recital of whose mechanical inventions are of

Huyghens. Reasons against Huyghens' claim to this invention.

 themselves

themselves alone sufficient to form a catalogue. Dr. Hooke is however allowed, even by foreigners, to have been the first who applied the pendulum spring to the balance of a watch; but this they say was straight in its form, and that it was M. Huyghens who afterwards made the great improvement in it, or rather invented it, by that of giving it the spiral form. This matter has been scrutinized before, and the dispute which Dr. Hooke had with Mr. Oldenburgh on this subject is very well known. Yet there are many circumstances that seem to have been overlooked, which carry along with them such strong arguments in favour of Dr. Hooke, that I am much surprized how they have been passed over and never noticed by any of his friends. Had M. Huyghens been the genuine inventor of the pendulum spring, which I confess, from all circumstances taken together, I think there are strong reasons to conclude against; had he been really the inventor, I say, it is much more than probable that he would have seen its properties as well as Dr. Hooke did, and would have published them; and this might have prevented the serious quarrel that afterwards took place between two very celebrated and rival French artists, M. Le Roy and M. Berthoud. Extracts from them on this subject of quarrel I shall afterwards give you, and in the meantime shall state Dr. Hooke's case, with extracts from him; which, although they came not out until immediately after M. Huyghens had published his account of the pendulum spring about the years 1674 or 1675, are sufficiently conclusive. Dr. Hooke was so much hurt with it, that he gave such a full account of his experiments, and so complete a demonstration of the principle or properties of springs, that it is evident, that the subject was not *new* to him.

Galileo invented or first recommended the pendulum.

It is necessary to pay attention to dates. Galileo died in 1642, and had given an account of the equality of the wide and narrow vibrations of the pendulum, and strongly recommended it to astronomers, as infinitely preferable to the balance, which they were attempting to use in their observatories. Riccioli pressed this matter exceedingly; and it came into general use as a measure of time, the astronomers patiently sitting by it and counting the vibrations.

Robert Hooke's introduction at Oxford.

In 1655, Mr. Robert Hooke came to Oxford as a poor scholar,

scholar, and brought with him a number of mechanical nick-nacks which he had made at home. His mechanical genius soon made him known to the members of the invisible society there, who employed him to work for them, making apparatus for their experiments. Dr. Ward, afterwards Bishop of Salisbury, took a liking to him, and instructed him in mathematics and astronomy. He urged him to try his mechanical genius in contriving a 'scapement pendulum. It would appear that they found the 'scapement for a balance, which had long been in use, did not answer, probably because it required very wide vibrations, which were found not so equable; and Mr. Hooke invented this sometime before February 1656; for there are observations of a solar eclipse made in that month, at Oxford, by a pendulum clock.

Attends to timekeepers.

Mr. Hooke got Riccioli's book from Dr. Ward, where mention is made of the proposal of Gemma Frisius to discover the longitude by a timekeeper; this he immediately proposed to do by a pendulum clock. But it is very remarkable, that Hooke had mathematics enough to see that even the smallest vibrations were not isochronous unless of equal width, although Galileo had asserted that no difference would be observed. Another remarkable instance of his great genius is, that though then only twenty-one years of age, he saw that every branch of human knowledge had a system of its own, and a set of principles on which it was regularly founded; and he said it was only by studying even *shoemaking* in this way, that one could be certain of improving it. He had already begun to form systems on the different subjects which had interested him. He called them algebras, because they enabled the possessor to invent and discover new things in their own line by rule, and with certainty of succeeding. Mechanics always was his favourite; and his mechanical algebra, or method of mechanic invention, he always considered as his greatest treasure. He says that no problem could be proposed to him in mechanics, but his algebra would immediately tell whether it was possible, and would put him and keep him in the right road for solving it. He told Dr. Ward that his algebra plainly showed him, that the only thing that could make equal vibrations, was an accelerating force proportional to the distance from the place of rest, and this was not true even of the smallest arches of a pendulum. But he had not mathematics enough

His systems for inventions called by him algebras.

He shows the law of isochronal vibrations;

enough to discover the cycloid, though he makes a most beautiful guess at it, and one of the best accounts of its properties that has been given. He says that the vibrations will be equal if you could make the pendulum move in such a curve as this: The small arches *ab, bc, cd, de*, &c. Fig. 2, Plate XI., being all equal, the perpendicular heights of them *Bb, Cc, Dd, Ee*, &c., must increase as the numbers 1, 2, 3, 4, &c. Now this is precisely the property of the cycloid discovered about eight years afterwards by M. Huyghens. Huyghens at the same time was speculating on this subject; and his father, who had been at Oxford, kept up a correspondence with the learned there. Huyghens being a consummate mathematician, soon investigated the motion of the pendulum, and by this time had conceived the project of getting a reward and a monopoly. His father was a member of the States General, and it was about this very year that they offered a reward of 10,000 florins. It is not improbable but he knew of his son's researches, and was a means of procuring this act of the States. About the year 1657, he (Huyghens) presented his first clock (not with a cycloid) to the States. It is not in the least probable, that a person, aiming at a monopoly and a public reward, would divulge his secrets, or that Mr. Hooke, at Oxford, would be informed of what he was doing. It is much more likely that the experiments and projects of the curious gentlemen at Oxford (who never made any secret of their operations, indeed durst not, because their meetings were suspected, and frequently visited by Cromwell's soldiers) might be known to Huyghens. We know for certain that Mr. Oldenburgh actually did communicate every thing to his countrymen. Be this at it may, Huyghens at this time thought of nothing but his cycloidal pendulum, nor for several years after. But in 1658, Hooke searched about for a force proportional to the distance from the place of rest, and found, experimentally, that *springs had this property*. He kept this a secret, but gave it to his friends in the following cypher: "*ee, ii i, n o, ss s, tt, uu*;" which was afterwards (in 1661) explained, "*Ut tensio sic vis*," *i. e.* As the tension, so is the force. He told Mr. Boyle that this was a secret for constructing pocket watches for the longitude, and showed him a watch, which was declared by Mr. Boyle to keep within half a minute a day; a thing infinitely beyond any watch then known.

—which was afterwards fully investigated by Huyghens.

The first clock of Huyghens. with a pendulum.

Hooke, in 1658, discovered the isochronism of springs.

Immedi-

Immediately after the Restoration, Mr. Boyle recommended Mr. Hooke to the Duke of York (who was very fond of sea affairs), to Lord Brouncker, and Sir Robert Moray, the most eminent at that time for mathematical knowledge; and proposed to Mr. Hooke to apply for a patent. Charles the Second founded the Royal Society, and enjoined the members to turn much of their attention to the improvement of navigation, and established the Greenwich Observatory for this very purpose; and the Parliament decreed a reward of £2000. Oldenburgh became secretary of the Royal Society, and kept a close correspondence with Huyghens, both public and private; and Huyghens was elected member in 1662 or 1663. He was in England in 1663, and was much caressed by all the learned, and particularly by those now named. During all this time there was not the least mention of his longitude watches; and Hooke's was kept a secret for reasons now to be explained.

Foundation of the Royal Society, &c.

Lord Brouncker, Sir Robert Moray, and Mr. Boyle were so much convinced of the superiority of Hooke's watches, that they aided him in procuring a patent. A warrant for one was actually signed by the king's orders for fourteen years.* Now these three gentlemen joined with Mr. Hooke in the prosecution of the affair, perhaps contributing the money wanted for carrying on the business of watch-making; and it was then that Mr. Hooke invented the engine for dividing and cutting clock and watch wheels now in universal use. Mr. Waller had several drafts of the mutual agreement, with various changes of the terms. It appears from them, that these gentlemen were to procure an act of Paliament for a duty of a groat per ton on all shipping sailing from any English port. And it was provided, that if the profits should exceed £6000, Hooke was to have three-fourths, and they to have the remainder. If it amounted only to £4000, he was to get only two-thirds, &c. It does not appear that all this while that Hooke disclosed his secret to them, further than by subjecting the watch's motion to their examination, along with Bishop Ward and Dr. Pell, and assuring them that the secret was contained in the cypher which he had long ago given to Mr. Boyle. But about the

History of Hooke's discovery of time-pieces, and treaty with Lord Brouncker, Sir R. Moray, and Boyle.

* This patent was in possession of Mr. Waller, secretary of the Royal Society in 1705.

end

end of 1660, it appears that things were brought to such a bearing that he explained the cypher, and even showed the construction of the watches. For in the register of the Royal Society it is recorded that Mr. Hooke had exhibited his *pocket* watches, which were *regulated* by springs. A pocket watch can be moved no other way but by a spring; and therefore the word *regulated* must undoubtedly apply or refer to the *regulation* of it by means of a pendulum spring. But the association now broke up. The three gentlemen insisted on another condition, that if they, or any other person, should remarkably improve this watch by any new principle introduced into it, they should be at liberty to enjoy the profits of the improvement even during the currency of the patent. This Hooke flatly refused, saying, that if once he showed them the principle, it was easy to improve on it; and he himself saw several imperfections in it, arising from the very nature of metals, which he was labouring to remove; and as he had no intention of excluding them from the benefits of any improvements of his, which might perhaps be still necessary before the watch was good for any thing, he would not be excluded from the advantages of any other improvements made on his invention. It is not unlikely that he had thoughts of the effects of heat and cold on the watch, and was thinking of adding a compensation piece of some sort or other to it.

He disclosed his secret (in part), and was afterwards unworthily treated.

By this means the affair of the patent broke up and miscarried; Hooke being exceedingly disgusted, and his manners being extremely ungracious and fretful, probably displeased the partners as much. He became extremely close and jealous after this.

The patent spring abandoned.

It was not till about 1674 or 1675, that Huyghens published his discovery of the spiral spring, applying both to the States of Holland and the Court of France, for such an extention of his patent as should comprehend watches. He was opposed from all quarters: the watchmakers allowed him to monopolize the pendulum, which they thought he had in some measure invented; but they did not choose his encroachment on watches. The Abbé Hautefeuille had also discovered the regulating power of a spring, and claimed the invention; his opposition was so effectual, that the registration of the French patent was stopped, but the Dutch patent was completed,

Disclosure of the invention of the balance spring by Huyghens: contested by Hautefeuille.

pleted, and M. Huyghens tried all methods to get it extended to England, but it was there opposed. M. Leibinitz was in Paris at the time of Hautefeuille's process, and says he was cast: this may be so, and still the patent might not be granted to M. Huyghens.

Communication of Hooke's discovery by Oldenburgh.

The first appearance of M. Huyghens' claim was about 1665. Sir Robert Moray, in a letter dated Oxford, 30th of September, 1665, to Mr. Oldenburgh, presumes, that from his intimacy with M. Huyghens, he will be among the first to hear of his watches, and desires him to ask him, whether he does not apply a spring to the arbor of the balance? This will bring M. Huyghens to say something of the matter; and if you find from his answer that this is the case, you may then tell him what Mr. Hooke has done in this way, and that he promises still more. Here it appears that Hooke's secret was in some degree known; and as Sir Robert had no longer any interest in the secret, he gives Oldenburgh leave to communicate it. Hooke complained much to the Society of these communications of their secretary.

Resumption: That Hooke appears to have invented the balance spring many years before Huyghens.

From this account, which is all founded on well authenticated facts, and does no great honour to the three gentlemen, it is plain, that Hooke had invented the regulating spring as early as 1658 or 1659; although perhaps he had not then given it that form which it now bears. His first watches were furnished with two cork-screw or cylindrical springs,* which acted on the balance arbor by a silk fibre or thread lapped round it. It is extremely probable that Huyghens knew of Hooke's discoveries in general, although it cannot be said with any certainty that he borrowed or stole the invention from him. Yet Hooke frequently charges him with this theft, but without being able distinctly to support the charge. Huyghens did not publish it till 1675, or thereabouts.

Hooke also invented the revolving pendulum, and a balance of the same kind.

These circumstances I think fully establish Hooke's claim to the invention of pendulum or balance springs; and that they were invented by him like a man of science, from principle, and not by chance discovered. I would also observe, that Mr.

* Cylindrical springs were used by Mr. Harrison in some of his essay timekeepers; and long afterwards, the late Mr. Arnold obtained a patent for them.

Hooke was long before him in the invention of the circular or conical pendulum, which he introduced for philosophical purposes, to represent the motion of the planets, and had proposed this pendulum regulated by springs instead of gravity, for a time measurer, before either Huyghens or himself thought of the balance spring. It was to consist of two balls, A B, exactly balanced round a centre, C, in the axis, C D, Fig. 3. Plate XI.: when this was set a whirling round the axis, the balls would fly out at right angles at once, but they were to be prevented by a spring, E F G, coiled round the centre, G,* and so tapered as to produce an isochronous circulation, although the maintaining power should vary the width of the revolutions. This was exhibited at Oxford, in 1657, but did not answer; but it shows that Hooke was well acquainted with the force and theory of springs. Nay, in 1660, he published his *Micrographia;* where there is occasionally mentioned a curious and paradoxial theory (as it then appeared) about the forces being as the squares of the velocities, instancing a great number of cases, among which are expressly mentioned bows and other elastic bodies, whose forces are proportional to their tensions. In 1676, Dr. Hooke published *A Description of Helioscopes and some other Instruments*, to which he has a *Postscript;* in which, among other things, he says, "At the "earnest importunity of a dear friend of mine since deceased, "I did, in the year 1664, read several of my first Cutlerian "lectures upon that subject (meaning the longitude) in the open "hall at Gresham College; at which were present, besides a "great number of the Royal Society, many strangers unknown "to me. I there shewed the ground and reason of that applica- "tion of springs to the balance of a watch, for regulating its "motions, and explained briefly the true nature and principle "of springs, to shew the physical and geometrical ground of "them. And I explained above TWENTY SEVERAL WAYS "by which springs might be applied to do the same thing, and "how the VIBRATIONS MIGHT BE SO REGULATED, as to "make their durations either all *equal*, or the greater, SLOWER, "or QUICKER than the less, and that in any proportion as-

Hooke, in his Culterian lectures, in 1664, explained numerous ways of applying springs to the balance, and rendering its vibrations equal in duration;

* Here we see the spiral spring applied to this machine; and it would be no difficult matter, after this, for Dr. Hooke to apply it to a watch balance.

"signed.

"signed. Some of these ways were applicable to lesser vibra-" tions, others to greater, as of 2, 3, 4, 5, 6, or whatever "number of revolutions was desired: the models of which I "there produced; and I did at the same time shew wherein "the aforesaid sea clocks (meaning Huyghens') were defec-"tive.

"All these particulars also were at several other times, at "the public meetings of the Royal Society, discoursed, experi-"mented, and several models produced. I did also, at the "earnest desire of some friends, in the years 1664 and 1665, "cause some of the said watches to be made, though I was un-"willing to add any of the *latter applications of the spring* to "them, as waiting a fitter opportunity for my own advantage."

—and before the Royal Society.

In 1678, Dr. Hooke published *Potentia Restitutiva,* or Spring; and says, "The theory of springs, though attempted "by divers eminent mathematicians of this age, has hitherto "not been published by any. It is now about *eighteen years* "since I first found it out, but designing to apply it to some par-"ticular use, I omitted the publishing thereof.

"About three years since, his majesty was pleased to see the "experiment, that made out this theory, tried at Whitehall, as "also my spring watch. About two years since, I printed this "theory in an anagram, at the end of my book of the *Descrip-"tion of Helioscopes*, viz. *Ut tensio sic vis.* That is, The power "of any spring is in the same proportion with the tension there-"of: that is, if one power stretch or bend it one space, two "will bend it two, and three will bend it three, and so for-"ward. Now, as the theory is very short, so the way of trying "it is very easy." Then he proceeds with describing his manner of proving both the *cylindrical and the flat helix*, and he even tried *straight wires.* The apparatus to which he applied his flat spiral springs, in order to prove or show their isochronism, differs little or nothing from the *elastic balance*, as M. Berthoud calls it, and which he boasts much of having invented, in order to prove *his theory of pendulum springs*, which he forms in such a way, that when bending them up equal degrees of tension, they shall have their forces in an arithmetical progression, which is just what Dr. Hooke, above an hundred years before, shows he had invented and done.

Other points of the history.

Dr. Hooke used an instrument for springs, since re invented by Berthoud.

Le Roy observed the property on which the isochronism of springs depends, 100 years after Hooke.

M. Le Roy the eldest, in his *Memorial on the best Manner of Measuring Time at Sea,* which was published in 1770, at the end of the *Voyage by M. Cassini,* gives us, among other improvements which he had made use of in his timekeeper, *one,* which regarded the isochronism of the pendulum spring, and which would have remained, as he says, hitherto unknown, had he not discovered this theory. His own words are. " It is " only some time ago, that I have at last discovered, as I shall " more particularly explain, this important fact, which hence-" forth must serve as a basis to the theory of watches, and as " a guide to the workmen to construct them ; namely, *that* " *there is in every spring of a sufficient extent, a certain length* " *where all the vibrations, great and small, are isochrone : that the* " *length being found, if you shall shorten this spring, the great* " *vibrations will be quicker than the small ones : if, on the con-* " *trary, you lengthen it, the small arcs will be finished in less time* " *than the large ones.* It is from this important property of " the spring, hitherto unknown, that particularly depends, as " we have by it seen, the regularity of my marine watch. " After what has gone before, we perceive that the justness of " watches depends in a great measure on the length given to " the spiral or regulating spring ; if with the same 'scapement " certain watches, or such as have, for example, the cylinder or " horizontal 'scapement, go ill, whilst that others of the same " sort are very regular, we here see the cause of it. This new " observation may be of great use in the construction of clocks, " whether small, or with second pendulums, where the pendu-" lum is suspended by a spring : indeed, we see from what has " been said, that there ought to be *there,* such a length in the " suspension spring, where all the vibrations of these pendulums " may be made isochrone."

Resumption of Hooke's claims.

It is evident, from what has been stated, that Dr. Hooke has the fairest claim to the honour of these important inventions and discoveries. I mean that of the watch pendulum spring, which he seems to have made in every *possible form.* And that he was master of the theory of springs, about which so much work of late years has been made, is equally evident. Among other reasons why he did not sooner publish an account of these inventions and discoveries, there is one, which though not generally known, it may be proper to mention here. After the dreadful conflagration, by which the greater part of Lon-

London was destroyed, 3d of Sept. 1666, Dr. Hooke was much engaged in surveying the waste ground left by the fire, and arranging the different claims and properties of the public and for individuals; by which it is probable he got much more money than he would have got by prosecuting the business of his longitude watches. I shall now give you some extracts from M. Berthoud, by which you will see what a serious quarrel took place between him and M. Le Roy, and, among other things, about the theory of pendulum springs, &c.

Account of the controversy between LeRoy and Berthoud respecting timepieces.

"I pass," says M. Berthoud,* "to a discovery of which M. "L. R. seems extremely jealous, that of the *isochronism by a* "*certain length of the spiral spring*, which he had proposed in "enigma, in 1768, in his *Exposé Succinct*, and which he "only divulged in 1770, in his *Mesure du Temps en Mer:* "he holds to it there, with so much the more reason, as he "is persuaded, that on this property of the spring, the regu- "larity of his marine watch particularly depended. I do not "dispute with M. L. R. that he has not discovered this "property of the spiral spring, by which *all the vibrations of* "*the balance* become *isochrone:* but I complain, and with "propriety and a just right, that he wants to dispute the dis- "covery of its having been made on my part; and dares to "accuse me of being only his copyist: it is very easy for me to "prove that I could not be so.

"The ill-timed jests that M. L. R. allows himself, and the "tone of raillery which he affects on this occasion, shall not "prevent the truth of that which, in my *Essai sur l'Horlogerie* "(tom i. page 168), I have said in speaking of my *elastic* "*balance*, and is this which follows:" "I had destined this "machine to make experiments on *the duration of great and* "*small vibrations of the same balance, which moves freely:* for "this purpose, I made the end of the pivot run on a very hard "stone; and to lessen the friction of the pivots, they each run "between three rollers. I observed the number of vibrations "which the balance made when it moved horizontally or ver- "tically, the velocity of the vibrations according to *the dif-* "*ference of temperature; and it must serve to measure the dif-*

* In his "Eclairssemens sur l'Invention et la Construction des "Horloges Marines."

"*ferent*

Account of the controversy between LeRoy and Berthoud respecting timepieces.

" *ferent degrees of force of the same spiral, accordingly as it is*
" *more or less bent up.*"

" The experiments which I pointed out are pretty clearly " designed; so much so, that they might assist artists " who know how to make use of materials when set before " them. *The duration of the great and of the small arcs of* " *vibration of the* FREE *balance,* and *the different degrees of* " *force of the same spiral accordingly as it is more or less bent* " *up.* Here is the origin of my theory on the *isochronism of* " *the vibrations by the spiral.* I had no occasion to make " use of it, when I wrote the first part of my *Essai sur* " *l'Horlogerie,* because the oscillations of the regulator, in " my first marine clock, were necessarily isochrone, from the " construction of the machine; first, because the 'scapement " corrected the inequality of the time or duration of the great " and small vibrations, which might result from changes in the " motive force, from variations of friction, and from the thick- " ening of the oil, &c. (*Essai,* No. 2116); secondly, because " the regulator being composed of two balances, the agitation " of the ship could not change the extent of the arcs of vibra- " tion, (see *Essai,* No. 2097). Thus, in all cases, the dura- " tion of the great or small vibrations must be equal, whether " by the assistance of the 'scapement, or by the nature of the " regulator. But since I had suppressed one of the ba- " lances, it became necessary to seek to correct the inequalities " of vibrations, which might result from the agitation of the " ship, and which were corrected by the double balance in " the first construction. I came back then to my original " ideas, and sought to correct by the spiral the alteration " which the agitations of the ship might produce on the extent " of the arcs, and on the inequality of the vibrations. Such " is the origin of my theory of the spiral: a theory which is " my own, as is obvious, and very easy to see; and I do not " dispute with M. L. R. his having made the discovery on " his part also. We must remark, however, an essential " difference between the importance which he attaches to " the property which the spiral spring has, of rendering the " vibrations isochrone, and the utility of which I have thought " that this property might be. It is *chiefly on the isochronism by*

" *the*

Account of the controversy between Le Roy and Berthoud respecting timepieces.

" *the spiral*, that M. L. R. founds, as he tells us, the just-
" ness of his marine watches; whilst I have never considered
" this property of the spring but as an useful accessary;
" and the justness of my marine clocks depends so little on
" it, that my clock, No. 8, whose spiral was not isochrone,
" has however succeeded very well in two trials of a year
" each. . . . .

" But I will suppose that I had not announced, in my
" *Essai*, the experiments which have led me to the disco-
" very in question; at least, M. L. R. will not deny that,
" *the 10th of February*, 1768, I deposited, or lodged with
" the Academy, my *new theory of the spiral*, in which I deduc-
" ed this proposition: *the oscillations of any balance whatever*
" *may be rendered isochrone by the spiral.* Nor can he deny
" that the publication of his *Exposé Succinct* was posterior
" to the date of my deposit; he would not then be well
" founded, to say that I have borrowed this theory from
" him, or the idea of the discovery, when even, as he falsely
" pretends, this discovery had even been divulged in his
" *Exposé Succinct.* But where do we find it *divulged* there?
" How has he announced it there? Here is all that he
" says of it (page 27 of the *Exposé Succinct):*" " I have
" discovered a property in the spring, by means of which
" I can easily come at the most perfect isochronism."

" What could these enigmatical words teach me? What
" is this *spring?* What is this property? We find, at the
" end of the *Exposé Succinct*, a copy of his project of 1754,
" in which he likewise said, that his balance would be sus-
" pended by a *straight regulating spring*, whose property
" was to render all the vibrations *isochrone.* Is it still
" about a *straight regulating spring* which M. L. R. would
" speak of in 1768?—or of a *spiral spring?* And I
" ask it of himself: Who could divine that these vague
" words, a *property in the spring*, announced *a certain*
" *length in the spiral spring?* But again, even if he should
" have announced his discovery clearly in the *Exposé Suc-*
" *cinct*, as he has lately done it in 1770, in his memorial
" on the *Measure of Time at Sea;* could I ever be sus-
" pected of having *copied* M. L. R. when I had deposited
" my

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" my discovery with the Academy before the *Exposé Suc-* " *cinct* was public.

" After these facts, which are notorious and well known to " every one, why has M. L. R. presumed to say what fol- " lows (page 18 of the *Precis*)?" " M. B. *having made the* " *discovery of the isochronism of springs a little later in-* " *deed; but if he had attempted to do it longer ago, it* " *would have been a hard task for him to show it, as is done* " *in my* Memorial; *it would be necessary for him to appro-* " *priate it to himself in some way, by the manner of pre-* " *senting it.*" " M. L. R. is undoubtedly persuaded that " such delicate raillery would impose on the public: but when " we shall have observed, that the *Memorial*, of which he speaks " here, is that of 1770; when we shall remark that he himself " agrees that I had deposited my discovery at the beginning " of 1768; when we shall have seen that, without taking much " trouble to notice it, he supposes that I could copy in 1768 " what he did not shew till 1770; I doubt whether the laugh " will be on his side.

" M. L. R. continues (page 18 of the *Precis*):" " *instead,* " *then, of resting the fact and its circumstances, such as the* " *experiment shewed them to be, M. B. gives the whole a* " *scientific air, of which it is not easy to comprehend any* " *thing.*"

" I am sorry that M. L. R. has not understood me, and I " will sincerely allow that it may be my fault. I am, how- " ever, encouraged by the praises which M. Daniel Bernouilli " has given to this part of my *Traité des Horloges Marines:* " here is what he wrote me concerning it at the beginning " of this year:" " *This article*" " (where I establish that the " force of the spiral must be in an arithmetical progression)" " *expresses perfectly the true principle of the isochronism.*" " And, in speaking of the experiments which I had made with " my elastic balance, he adds," " *These experiments are cer-* " *tainly of infinite interest; they confirm the true principles* " *of the isochronism, and shew the limits of the greatest ex-* " *tent that we can give to the balance without injuring the* " *principle: this is where the progression of small weights* " *begin to descend in an arithmetical progression, &c.*" " I

" confess

"confess that I am more flattered that M. Bernouilli has "understood me, and has approved of me, than if I had "only been barely understood by M. L. R.

Account of the controversy between LeRoy and Berthoud respecting timepieces.

"Although M. L. R. has not understood me, he under-"stands how to reason upon my theory, and censures the "course that I have taken." "Mr. B." "says he (page 18 "of the *Precis)*," "supposes, from the beginning, that the "force of long and weak springs increases in a less ratio than "the spaces described in its different tensions, since he con-"cludes from it that the great vibrations are, in this case, "slower than the small ones, and *vice versa* for the short and "strong spring: but *he should not suppose, he should show that* "*the things are so in nature, of which it is only experiment* "*can instruct us.*"

"I confess, that my course has always been the opposite "of the precept of M. L. R. In all my researches, I have "begun by adopting or assuming principles: I have endea-"voured to sift these well, and have called experiments to "my assistance with a view to confirm these principles. It "is true, that by this method we lose the advantage of "meeting sometimes with lucky chances, which discover "what we were not seeking; but, in return, when we have "made a discovery, we know to what principle we must "attribute it, and are not in the state of him who lends one "to it purely imaginary.

"M. L. R. is not satisfied with attacking me on my pro-"perty in my theory; he wants even to attack the solidity of "it. I have defended this property by facts, which prove "that I could not know the researches of M. L. R. I shall "now defend the solidity of it by reasons, at the risk of not "being understood by the author of the *Precis*.

"The criticism that M. L. R. has made on my theory, "obliges me to enter here into some discussion. It is ne-"cessary, first, to bring under one point of view all that he "has said on the *isochronism by the spiral*, in his *Mémoire* "*sur la Mesure du Temps en Mer*, printed in 1770, at the "end of the *Voyage de M. Cassini fils*.

"*I have always discovered*," (says M. L. R.) "*as the* "*most famous philosophers and artists have done, that the*

Account of the controversy between LeRoy and Berthoud respecting timepieces.

" *great vibrations were slower than the small ones.*" " (page " 14 of the *Mesure du Temps en Mer*).

" I have, in general, proved the contrary, and shall adduce " proofs of it hereafter."

" I have likewise remarked" (adds he), " that on a double " arc the difference was for the most part $\frac{1}{120}$th. *This effect* " *comes, I believe, from the mass of the spring when bending* " *and unbending, or perhaps from the obstacles that it finds* " *internally when bending and unbending itself.*" (Ibidem).

" What are these internal obstacles? This explanation does " not seem very intelligible. M. L. R. must undoubtedly be " understood; the internal properties of the spring must be " known to him; since, from 1750, he had announced to " us that he would forthwith give" " *A complete Treatise on* " *the Nature of the Spring, and on its Effects, &c.*"

" M. L. R. continues:" "*It is only very lately that I* " *have discovered, as I shall explain it more particularly,*" " (We find this explanation no where in his *Mémoire*)." " *this fact, so important, which henceforth must serve as the* " *basis of the theory of watches, and as a guide to work-* " *men; to wit, that there is in every spring of a sufficient ex-* " *tent, a certain length, where all the vibrations, great and* " *small, are isochrone.*"

" First—What is this extent? Secondly—This proposi- " tion is not generally exact; for we find a great number " of springs, which are such by their nature, that whatever " be their *sufficient extent*, they never will be isochrone.

" *I have discovered,*" (adds M. L. R.) " *that this length* " *being found, if you shorten the spring, the great vibra-* " *tions will be quicker than the small ones. If, on the con-* " *trary, you lengthen it, the small ones will be finished in* " *less time than the great ones.*"

" This second part of the proposition of M. L. R. is ge- " nerally exact; and on this point we are agreed."

" *It is*" (adds he) "*from this important property, hither-* " *to unknown, that the regularity of my marine watch par-* " *ticularly depends.*"

" M. L. R. thinks that this property was universally *un-* " *known*; and it would have been indeed so, if it had been " only

"only known by what he said of it before 1770. But I "have proved that I did know it; since, as we have "seen, that on the 10th of February, 1768, I had lodged "with the Academy my *Theory of the Isochronism of the* "*Spiral*.

Account of the controversy between LeRoy and Berthoud respecting timepieces.

"Is (it there) all that M. L. R. has told us, in 1770, of "this property of the spiral spring, in his *Mémoire sur la* "*Mesure du Temps en Mer*, which contains the description "of his present watches. I have sufficiently proved by the "dates of our productions that I could not be his copier; "but I can yet prove in another manner that I could not "be so, since we agree not, either in the fundamental prin-"ciple of our theory on the isochronism by the spiral, or in "all its consequences.

"First, M. L. R. says, that in all the experiments which "he has made on the time or duration of the vibrations of a "balance with the spiral spring, he has almost always found "that *the great vibrations are slower than the small ones.*

"All the experiments, on the contrary, which I have re-"lated in my *Traité des Horloges* on spiral springs, and "a still greater number which I have made, and which "are not mentioned in that work, prove that, in general, "the spiral renders *the great vibrations* of the balance "*quicker than the small ones.* See, in the *Treatise on Ma-*"*rine Clocks*, the experiments of No. 137, 206, 212, 215, "216, 217, 218, 219, 220, 225, 227, 228, 230, 232, the "first of 233, and the No. 234, 928. The experiments of "207, 221, 226, and the second of 233, are the only "ones which could give the great vibrations slower than the "small ones: and still it is only by a long and difficult "task that spirals can be brought to that point which alone "can assure us, that the spiral is susceptible of being made "*isochrone*, a property which we obtain then by shortening "it. Less fortunate than M. L. R., who tells us (page 34 "of his *Mémoire*) that "*this operation* (of seeking the point "where a spiral is isochrone) *seemed at first tedious, but* "*that practice renders it so easy, that at once he now knows* "*pretty nearly the length of the spring where all the vibra-*"*tions are of equal duration.*" "I confess, on the contrary, "that, though aided with an excellent instrument in my

Account of the controversy between LeRoy and Berthoud respecting timepieces.

" elastic balance, it is only with much trouble that I have " found some spiral springs fit to succeed by their isochron- " ism. And again have I found them quite altered on at- " tempting to *temper them when turned up*. This operation " is, however, indispensable, if we want to give them the " quality of keeping a constant figure, a quality that is pre- " ferable by much to that of the isochronism by the spi- " ral, in machines destined to undergo all the changes of " temperature, which never fail to alter the figure of springs " when they have not been tempered after being turned up.

" I must add, that if my experiments do not agree with " those of M. L. R. they agree with those of Mr. Harri- " son: this celebrated artist always found, as well as I did, " that *the great arcs of vibration were quicker than the small* " *ones*.

" M. L. R. after having set out with a principle contra- " dicted by experiment, makes the isochronism to consist " only in the more or less of length of the spiral spring; " whereas I have proved in my *Traité des Horloges Marines*, " first, that we can arrive at isochronism without rendering " the spring longer, but by making it *broader and thinner*, (No. " 143). Secondly, that we can come at it by a great num- " ber of close turns, (No. 154); or by rendering the spring " *stronger or weaker at the centre or outwards*, (No. 157): " I have shewn, that the lamina or wire must be made " like a whip or lash, strongest at the centre, (No. 159). " See also the Nos. 159, 222, 235, &c.; and, in general, " see in the table of matters of the *Treatise of Marine Clocks*, " at the word *spiral*, all the articles where it is treated of.

" The quality of isochronism is precious, without doubt, " in a spiral spring, and I have insisted on it in my *Traité des* " *Horloges Marines*, not, as M. L. R. pretends, because the " justness of my marine clock is founded, like that of his " watches, on the isochronism of the spiral; not because I " believe that, *without this method, we shall ever have only* " *feeble success in marine clocks*. I have neither said nor thought " so; but because this method can render the making, regu- " lating, or timeing of these machines, more expeditious and " more easy. I have always looked on it merely as an useful " accessary, which, perhaps, might render my clocks still

" more

" more perfect. But two indispensable qualities in the spi-
" ral are, first, the *constancy of force*, (abstraction being
" made of the accidental changes produced by the action
" of heat and cold); secondly, the *constancy of figure;* and
" it is on these two qualities, of the first necessity, that I have
" always insisted, and that I still insist on.

M. Le Roy gave the isochronism to the balance by a certain length in the flat spiral spring: and M. Berthoud, to obtain the same, condemns the cylindrical helix as being unfit for this purpose, and uses the flat helix tapered thinnest outward. Mr. Arnold used the cylindrical helix; and Mr. Earnshaw recommends tapering the spring thinnest inward. These diversities of opinion still serve to confirm what Dr. Hooke observed in the numerous experiments which he made with springs; namely, that he could obtain the isochronism by twenty different ways.

As you had requested me, I shall now endeavour, Mr. Nicholson, to furnish you with some accounts of the detached 'scapement, and shall give you, as far as I have been able to discover it, the history of *its invention* and subsequent progress.

Observations of Berthoud respecting the first free escapement.

" I pass" (says M. Berthoud *) " to the detent 'scapement
" having free vibrations (the detached), over which M. L. R.
" pretends to have such an incontestible and exclusive right."
" M. B." (says he) " relates, that the late *M. de Camus* had
" told him that the deceased *M. du Tertre* was the first who
" had this idea. He assures us besides, that in 1754, he him-
" self had contrived one of this sort; and that when in Lon-
" don, in 1766, Mr. Mudge had shewn him one similar, or
" nearly so. We perceive clearly for what purpose he makes
" all these quotations, but that they may only prevent the
" truth being known, of what, in 1748, the Academy had de-
" clared, in speaking of the first 'scapement which had
" appeared with free vibrations, and which I had presented to
" it, that the idea of it seemed new to the Academy, and
" susceptible of many advantages."

* In his " Eclairssemens sûr l'Invention et la Construction des " Horloges Marines."

It

Farther particulars and remarks.

" It is very true, that in 1748, M.L.R. presented a 'scapement " to the Academy; it is very true, that the Academy said then, " that the idea of it seemed new. What was this 'scapement? " The Academy does not tell. But can M. L. R. assure us " now, supposing that this was a 'scapement with free vibra- " tions, that the idea of it was *new?* Can he require that we " should believe that his 'scapement of 1748 was *the first* of this " sort *which had then appeared?* And could it be possible that " he had forgot, what he has himself said of it, in his *Etrennes* " *Chronométriques* for the year 1759? I shall now set his own " words before him." "Convinced" (said he) "of the verity of " the sentiment of *Descartes*, I undertook, in 1751, to make " a clock to go eight days with one wheel only in the movement. " What gave me the idea of this construction, was the 'scape- " ment of a watch with a rest or dead-beat, and a *detent*, " which I presented to the Academy in the year 1748, with " whom it carried or received a favourable opinion, as may " be seen in the Memoirs for that year. *My contrivance was* " *not so new as I had imagined: M. M. du Tertre's sons, con-* " *siderable artists in many respects, shewed me, soon after, a* " *model of the watch of their late father's, and which the* " *eldest M. du Tertre must still have.* This model, though " very different from my construction, is, however, the same " as to the end proposed; the motion in both is only re- " stored to every other vibration, &c. And lower down we " read what follows:" "The liberty or freedom procured to " the regulator in the 'scapement of M. du Tertre, by *a* " *detent* formed like a long lever, which was stopped * dur- " ing two vibrations by the arbor of the balances, and " moved by an anchor, seemed to me at that time very " advantageous, &c."

" The late M. Camus was not then so very wrong, when " I shewed him, in 1754, my 'scapement having *a detent* " *and free vibrations*, in telling me, that long ago the de- " ceased M. du Tertre had proposed and made use of one " like it. We shall find ourselves, M. L. R. and me, in

* The description of Du Tertre's 'scapement, as given here by Le Roy, is unintelligible and obscure. Du Tertre was much engaged in improving 'scapements about the year 1724.

" the

" the same situation, by being left behind in an invention " which had presented itself long before to several artists; " but M. L. R. wants to appropriate it exclusively to " himself; and, on the contrary, I have done homage to " M. du Tertre, as to him who had the first proposed it; " although, assuredly, I had no knowledge of any 'scapement " of this sort when I proposed mine, executed and in a " finished state, to M. Camus. We readily perceive why " M. L. R. is so jealous of this invention: he is persuaded " that *it is by the 'scapement that the most part of the* " *trials have miscarried which have been made to discover* " *the longitude by timekeepers*, and that the 'scapement, with " the detent and free vibrations, is exempt from all faults. " I am very far from thinking on this subject as he does; but " this is not the place to enter into a discussion, which would " lead us too far. I believe, moreover, that we might make " use of, and with equal success, 'scapements of a very differ- " ent nature: and this is not an opinion, it is a fact proved by " experience. The marine watch of Mr. Harrison, that of " M. L. R., and my clock, have each a 'scapement, which " differ essentially from one another, both in their principle " and in their action. Moreover, I shall make no hesitation " to use the 'scapement with free vibrations, if the experiments, " which I propose to repeat, ever prove to me that it is prefer- " able to any other; and in that I will make use of it as my " own right, and shall not think or believe myself to have co- " pied, in any manner, neither that of M. L. R. nor that of " M. du Tertre, for I know not the construction of the 'scape- " ment of this last; and those of this kind, which I have con- " trived, may be seen in my *Traité des Horloges Marines*, " differing from the 'scapement of which M. L. R. has given " us the description of in his *Mesure du Temps en Mer*. I " will likewise not dispute against the preference which " M. L. R. thinks that his 'scapement ought to have over " mine; it is so natural to love our children even when they " are only adopted.

Different escapements have performed well.

M. Berthoud, in his *Supplément au Traité des Horloges Marines*, says, that he executed five marine clocks having the 'scapement with the spring detent, *(détente-ressort)*, which were begun in 1768, and completed in 1782. Those who

Timepieces of Berthoud.

who wish to see more on the subject of the isochronism of pendulum springs, and of detached 'scapements, may consult his *Traité des Horloges Marines*, published in 1773, and the *Supplément au Traité des Horloges Marines,* published in 1787. There is, in Thiout, an idea of a sort of detached 'scapement; this work of his was published in 1742.

In M. Berthoud's *Supplément,* he says, "Such are the ob-"servations which I made the 17th of March, 1763, in com-"posing my first astronomical watch, which was finished "about the beginning of 1764, the designs and the description "of which were lodged with the Academy in August 1764." "See *Traité des Horloges Marines*, Append. page 533." "I sold "this watch, when in London, in 1766, to Mr. Pinchbeck, who "purchased it for the king of England: it was the first pocket "watch that had been made in Europe having a compensation "for heat and cold."

Le Roy's balance of compensation by solid parts,

M. Le Roy, in his *Mesure du Temps en Mer,* gives the drawing of a compensation balance, which is the first,* as it were, that I have seen or heard of, the idea of which he confesses to have taken from Mr. Harrison's compensation bar. But the invention of the compensation balance *itself* is due to Mr. Harrison, which

— is presumed to have been previously made by Harrison.

may be presumed from what follows: "You will now permit "me to speak a word or two, as to the compensation for heat "and cold in the balance. It is the original method by which "Mr. Harrison attempted to correct the error, which, as he "was pretty tenacious of his own opinion, he carried into exe-"cution contrary to the advice of Mr. Graham, but found, "by experience, that Mr. Graham was right, and was forced "to throw it all away,† and to contrive his method of apply-"ing it to the balance springs." See Mr. Mudge's Letters to Count Bruhl.

Great merits of Arnold.

And farther, in honour of our country, it must be acknow-

* I suppose this expression to imply the compensation balance of two metals, in contradistinction to the fluid thermometer balance, which has no relation to Harrison.—N.

† The late Mr. Arnold, by making the compensation in the balance, and by its being now carried into general practice, and, as it were, confirmed, proves that Mr. Harrison's original idea was good.

ledged

ledged that the late Mr. Arnold, as well for his own inventions as from his ability for improving whatever came before him, deserved all the encouragement he met with from his private business, and from the public rewards which he may from time to time have obtained, if it was no more but for his exertions in persevering and shewing a track which others might follow; and this he did with an enterprizing and ingenious spirit, of which few men were capable. All circumstances considered, the business of making chronometers stands more indebted to him than any other man since Dr. Hooke; with whose merits Mr. Arnold was so well acquainted, and whom he thought so much of, as to set him on a par, nay, even above the most celebrated Sir Isaac Newton. When Mr. Arnold began his career in life, he was the first who brought watch jewelling, and the application of stone to the places of action, into more general use than ever had been done before his time; and, although *these* may not have any thing of a mechanical principle in them, yet they certainly render any principle connected with the holes, &c. as, for example, the pitchings and 'scapements are made more permanent than they would otherwise be, by which our watches have acquired a stability and character (from jewelled holes, &c.) that in all probability they would not have had without them. I speak from experience of the utility of these things; the pallets of my astronomical clock being of stone, and it has been going with me for sixteen years, without the smallest application of oil to them in any manner whatever.

Arnold extended the practice of jewelling.

I am, Sir,

With much respect,

Yours,

THOMAS REID.

Edinburgh, July 12, 1806.

P. S.—In the work published by M. Thiout, in 1741, there is a large collection of the various 'scapements then known; and, among others, there is one which seems to have been invented by himself, and is a sort of free or detached one, the same in its principle as the one invented by M. Le Roy in

A detached escapement by Thiout, in 1741, prior to Le Roy's, and not essentially different.

1748. It may be said to operate thus: After one of the balance wheel teeth has given impulse to the pallet, and has just quitted it, there is a *detent*, which is made to come in and catch or stop the wheel, by interposing itself in the way of one of the wheel teeth. Here the wheel rests during this vibration; and, on the balance returning, there is on its axis an arm or *pallet*, which gets into a sort of fork fixed on the arbor of the detent, and which serves, by means of this fork, both to lock and unlock the wheel. The unlocking is never completely done until such time as the pallet (which receives the impulse) has got near or against the point of the next impelling tooth, which then gets a recoil from the pallet, by the momentum of the balance on its return, and on the wheel being free, and again pressing the pallet forward, it again becomes locked, and so on.

Le Roy and Berthoud must both have known Thiout's escapement.

How has it happened, that both M. Berthoud and M. Le Roy have overlooked this 'scapement of M. Thiout's? It could hardly be unknown to them in the course of their dispute concerning the priority of invention of the free or detached 'scapement.

Description.

In the figure, Plate X. Fig. 1., A B is the balance wheel; G the pallet in which the teeth act; *d* the arbor of the detent; *e* the locking and unlocking pallet, which is done by means of the fork *f*; *h* the hook which locks the wheel teeth; *i* is the verge or axis of the balance. M. Le Roy made the balance wheel a sort of contrate one, having the teeth standing upright. This is a small improvement on M. Thiout's, which seems to be also the origin of the *Echappement à Virgule*.*

* Some of the information contained in the above letter has been, by a singular coincidence, anticipated in an article in the last number of this Journal. It may, therefore, be necessary to mention, that what regards Dr. Hooke in the present letter, was composed fourteen years ago, and that the whole was in possession of Mr. Nicholson previous to the publication of that essay.—*Note of the author.*

III.

A Method of rendering all the Vibrations of the Balance of a Timepiece equal. In a Letter from Mr. WILLIAM HARDY.

To Mr. NICHOLSON.

SIR,

I BEG leave to communicate to your readers a new and easy method of correcting the long and short arches of vibration in the balance of a timekeeper; that is to say, that they shall all be performed in equal times, so short as the angle of escapement will admit.

New method of connecting the vibrations of a balance. The stud is a spring which allows play endwise.

The spring, *a b*, Fig. 4, 5, 6, Plate XI., screwed to the under part of the cock, *h*, lies over the upper part of the pendulum spring, proceeding in a right line to the axis, *e*, of the balance, *g*, having a bend to clear the verge, and so passes on to the other side, where the end of the pendulum spring is fastened to it. This straight spring is reduced to such a consistency, as to allow it to be brought into action a little before the pendulum spring. The other piece, *c*, which projects down from the under side of the cock, lies in a line with this spring, and is screwed to the cock on the opposite side of the centre to that where the spring is fastened. It is of an oblong form, and has a slit cut down, with an adjusting screw, *d d*, on each side, whose points face each other in the same right line to receive the small projecting piece, *b*, which is at the end of the straight spring, so as to move freely between them. The points of the screws should be at equal distances from the spring. When the balance is at rest, the space between the two screws must be considerably less than the angle of 'scapement, but the proper quantity must be determined by trial. As this straight spring is weaker than the pendulum spring, it will be first brought into action; therefore, if the balance be made to move only so far as to cause the spring to act between the two adjusting screws, the motion of the balance will be prolonged; but on being stopped by the adjusting screws, the action of the straight spring will cease, and that of the pendulum spring will commence, and consequently progressively accelerate the vibrations of the balance. It will, therefore,

always

always oppose the accelerating effects of the wheel in the short vibrations, and so cause the whole of them to be performed nearly in the same time.

I am, Sir,
Your most obedient humble servant,
WILLIAM HARDY.

IV.

Remarks on Achromatic Eye-pieces. By DAVID BREWSTER, *A. M.*

To Mr. NICHOLSON.

SIR,

Introductory remarks.

I OBSERVE, in the last number of your Journal, a query respecting a rule for achromatic eye-pieces, contained in my appendix to *Ferguson's Lectures*. Your correspondent seems to question the accuracy of that rule, and to imagine that no combination of lenses whatever can form an eye-piece capable of removing the chromatic aberration. Were this principle to be admitted, the rules which I have given for achromatic eye-pieces, composed of three or four lenses, would likewise be inaccurate; and the ingenious eye-pieces by which Dolland and Ramsden have rendered their telescopes superior to those of every-other artist, would be liable to the same imputation. I could easily demonstrate, were it necessary, that an eye-piece consisting of two lenses, whose focal lengths, reckoning from that next the object, are as the numbers 3 and 1, and whose distance is equal to the difference of their focal lengths, will almost wholly remove the aberration of colour. But I imagine it will be a sufficient answer to the query of your correspondent, and a sufficient vindication of the rule to which he alludes, if I can explain to him the reason why the error, arising from the different refrangibility of the coloured rays, may be corrected by a judicious combination of lenses of the same refractive and dispersive power.

Explanation of an achromatic eye piece.

Let A B, Fig. 1. Pl. XI., be an achromatic object-glass, and D, E two lenses of the same kind of glass. Let C D E be the axis

axis of the telescope, and, R S, a ray passing through the centre of the object-glass. This ray will fall, in an uncompounded state, upon the eye-glass, D, even if the object-glass were not achromatic; for, as it passes through its centre, it undergoes two equal and opposite changes, and is therefore not separated into the prismatic colours. After refraction through the lens, D, the rays, R S, will be decomposed, and the red portion of it will meet the axis in *r*, and the violet ray will meet the axis in *v*. But when the second eye-glass, F, is interposed, the red ray will meet its anterior surface in *m*, and the violet ray at a point, *n*, nearer the axis. Now, as the refracting angle of the lens is greater at *m* than at *n*, the red ray, *S m*, will be more refracted than the violet ray, *S n*; and this increase of refraction will compensate for its inferior degree of refrangibility. From this cause the refraction of the second lens, E, will render the resulting rays, *m r'*, *n v'*, parallel, and thus destroy the chromatic aberration, which is always proportional to the angle formed by the resulting rays, *m r'*, *n v'*. In order that distinct vision may be obtained with this eye-piece, the rays must fall converging on the first eye-glass, D. This can be effected only by placing the lens, D, between the object-glass and its principal focus, and by this means the telescope is rendered shorter than if only one convex lens had been employed. The equivalent lens, or the lens which would produce the same magnifying power as this eye-piece, is equal to *half* the focal length of the glass, D.

I am, Sir,

Your obedient humble servant,

Mount Annan, July 21, 1806. DAVID BREWSTER.

V.

Chemical Experiments on Guaiacum. By Mr. WILLIAM BRANDE. *From the Philosophical Transactions*, 1806.

AMONG the numerous substances which are comprehended under the name of resins, there is perhaps no one which possesses so many curious properties, as that now under consideration; and it is remarkable that no more attention has been

Guaiacum possesses very singular properties.

been paid to the subject, since many of the alterations which it undergoes when treated with different solvents, have been mentioned by various authors.

External and obvious characters of guaiacum.

SECT. I.—Guaiacum has a green hue externally; is in some degree transparent; and breaks with a vitreous fracture.

When pulverised it is of a gray colour, but gradually becomes greenish on exposure to air.

It melts when heated, and diffuses at the same time a pungent aromatic odour.

It has when in powder a pleasant balsamic smell, but scarcely any taste, although when swallowed it excites a very powerful burning sensation in the throat.

Its specific gravity is 1.2289.

Aqueous solution imperfect.

SECT. II. 1.—When pulverised guaiacum is digested in a moderate heat with distilled water, an opaque solution is formed, which becomes clear on passing the whole through a filter.

The filtrated liquor is of a greenish-brown colour; it has a peculiar smell, and a sweetish taste.

It leaves on evaporation a brown substance, which is soluble in alcohol, nearly soluble in boiling water, and very little acted upon by sulphuric ether.

This solution was examined by the following reagents.

Filtered solution examined by reagents.

Muriate of alumina occasioned a brown insoluble precipitate after some hours had elapsed.

Muriate of tin formed a brown flaky precipitate under the same circumstances.

Nitrate of silver gave a copious brown precipitate.

Suspecting the presence of lime in the solution, I added a few drops of oxalate of ammonia, when the liquid immediately became trubid, and deposited brown flakes, which, after having been treated with boiling alcohol, yielded traces of oxalate of lime.

These effects, therefore, indicate the presence of a substance in guaiacum, which possesses the properties of extract;* the

* By the terms extract, I mean that substance, which by chemists is called the extractive principle of vegetables. Vide Thompson's Syst. of Chemistry, 2d edit. vol. iv. p. 276.

action

action of the reagent is however somewhat modified, by a small quantity of lime which is also in solution.

One hundred grains of guaiacum yielded about nine grains of this impure extractive matter.

2. Alcohol dissolves guaiacum with facility, leaving some extraneous matter, which generally amounts to about 5 *per cent.*

Alcholic solution of guaiacum considerably perfect.

This solution is of a deep brown colour; the addition of water separates the resin, forming a milky fluid which passes the filter.

Acids produce the following changes:

A. Muriatic acid throws down an ash-coloured precipitate, which is not re-dissolved by heating the mxiture. In this case the resin appears but little altered.

Changes produced by acids:

B. Liquid oxy-muriatic acid when poured into this solution, forms a precipitate of a very beautiful pale-blue colour, which may be preserved unaltered.

C. Sulphuric acid, when not added in too large a quantity, separates the resin of a pale green colour.

D. Acetic acid does not form any precipitate. This acid is indeed capable of dissolving most of the resins.

E. Nitric acid diluted with one-fourth of its weight of water, causes no precipitate till after the period of some hours. The liquid at first assumes a green colour, and if water be added at this period, a green precipitate may be obtained; the green colour soon changes to blue, (when by the same means a blue precipitate may be obtained); it then becomes brown, and a brown precipitate spontaneously makes its appearance, the properties of which will be afterwards mentioned.

The changes of colour produced by nitric, and oxy-muriatic acids, in the alcoholic solution; are very remarkable, and I believe peculiar to guaiacum: there is moreover much reason to suppose that the above alterations in colour are occasioned by oxigen * It likewise appears from that which has been stated,

—very remarkable, and peculiar to Guaiacum.

* The following experiments appear to verify this supposition:

Fifty grains of freshly pulverised guaiacum were introduced into a glass jar containing 60 cubic inches of oxy muriatic acid gas. The resin speedily assumed a brown colour, having passed through several shades of green and blue. Liquid ammonia was poured on this brown substance, while yet immersed in the acid; the whole became

stated, that the blue and green oxides (if they may be so called by way of distinction) are soluble in the mixture of nitric acid and alcohol, while the brown precipitate is insoluble.

Alkalis do not precipitate the alcoholic solution.

F. Alkalis do not form any precipitate when added to the solution of guaiacum in alcohol.

Direct action of ether and of the acids upon Guaiacum.

3. Guaiacum is less soluble in sulphuric ether than in alcohol; the properties of this solution nearly coincide with those just mentioned.

4. Muriatic acid dissolves a small portion of guaiacum, the solution assuming a deep brown colour; but if heat be applied, the resin melts into a blackish mass, preventing any farther action from taking place.

5. Sulphuric acid forms with guaiacum a deep red liquid, which, when fresh prepared, deposits a lilac coloured precipitate on the addition of water; a precipitate is also formed by the alkalis. If heat be employed in forming this solution, the resin is speedily decomposed; and if the whole of the acid be evaporated, there remains a black coaly substance, together with some sulphate of lime.

Action of nitric acid upon guaiacum.

6. Nitric acid appears to exert a more powerful action on guaiacum than on any of the resinous bodies.

100 grains of pure guaiacum previously reduced to powder, were cautiously added to two ounces of nitric acid, of the specific gravity of 1.39. The resin at first assumed a dark green colour, a violent effervescence was produced, attended with the emission of much nitrous gas, and the whole was dissolved

became green; it therefore seemed thus to be deprived of part of the oxygen which it apparently had acquired by the preceding experiment. An equal portion of the same guaiacum was exposed under similar circumstances to the action of oxy-muriatic acid, excepting that the glass in which the experiment was made, was covered with a black varnish, and placed in a dark apartment. On examining the result of this experiment, the resin was found to have undergone precisely the same changes as when exposed to light. Ammonia had also the same effect.

Guaiacum was also exposed over mercury to oxygen gas; the resin assumed after some days the green colour which a longer exposure to the atmosphere produces: this change was likewise found by a second experiment to be effected without the presence of light.

with-

without the assistance of heat, which is not the case with the resins in general; for when these bodies are thus treated with nitric acid, they are commonly converted into an orange-coloured porous mass.

The solution thus formed, yielded while recent, a brown precipitate with the alkalis, which was redissolved on the application of heat, forming a deep brown liquid.

Muriatic acid also separated the guaiacum from this solution, not however without having undergone some change.

Sulphuric acid caused no precipitate.

After this solution of guaiacum in nitric acid had remained undisturbed for some hours, a considerable proportion of crystallized oxalic acid was deposited

When guaiacum was treated with dilute nitric acid, the results were somewhat different. A slight effervescence took place, and part of the resin was dissolved, the remainder being converted into a brown substance, resembling the precipitate obtained from the alcoholic solution as above mentioned. (2. E.) Dilute acid.

This brown substance appears to be guaiacum, the properties of which are materially altered, by its combination with oxigen; and I am led to think that the changes of colour produced by nitric and oxy-muriatic acids, are the consequence of the different proportions of oxigen with which the guaiacum has been united; for we know that the colours of metallic, and many other bodies, are greatly influenced by the same cause.

The brown substance was separated by filtration; the filtrated liquor yielded yellow flocculent precipitates with the alkalis, and on examination was found to hold nitrate of lime in solution.

The undissolved portion was of a deep chocolate-brown colour. A similar substance may also be obtained, by evaporating the recent nitric solution to dryness, taking care not to apply too much heat towards the end of the process.

The substance obtained by either of these means, possesses the properties of a resin in greater perfection than guaiacum; it is equally soluble in alcohol and sulphuric ether, insoluble in water, &c.; but when burned it emits a peculiar smell, more resembling animal than vegetable bodies. If, however, fresh portions of nitric acid be added three or four times succes-

 sively;

sively; or if a large quantity be employed to form the solution; the product obtained by evaporation is then of a very different nature; for it has lost all the characteristic properties of a resin, having become equally soluble in water and alcohol; the solution of it in this state having an astringent bitter taste.*

Alkalis and their carbonates dissolve guaiacum:

7. Guaiacum is copiously soluble in the pure and carbonated alkalis, forming greenish-brown liquids.

Two ounces of a saturated solution of caustic potash took up rather more than 65 grains of the resin; the same quantity of liquid ammonia dissolved only 25 grains.

—precipitable by acids.

Nitric acid formed in these solutions a deep brown precipitate, the shades of which varied according to the quantity of acid which had been employed.

This precipitate was found on examination to possess the properties of that formed by nitric acid in the solution of guaiacum (2. E.) in alcohol.

Dilute sulphuric acid, when poured into any of the above alkaline solutions, formed a flesh-coloured curdy precipitate. Muriatic acid produced the same effect.

The two last-mentioned precipitates differ from guaiacum, in being less acted upon by sulphuric ether and more soluble in boiling water: their properties therefore approach nearer to extract. Moreover, when these precipitates were dissolved in ammonia, and were again separated by muriatic acid, the above-mentioned properties became more evident.

Products of distillation upon guaiacum.

Sect. III.—100 grains of very pure guaiacum in powder, were put into a glass retort, to which the usual apparatus was adapted. The distillation was gradually performed on an open fire, until the bottom of the retort became red hot.

The following products were obtained:	grains.
Acidulated water	5.5
Thick brown oil, becoming turbid on cooling	24.5
Thin empyreumatic oil	30.0
Coal remaining in the retort	30.5
Mixed gases, consisting chiefly of carbonic acid and carbonate hydrogen	9.0
	99.5

* Vide Mr. Hatchett's two papers on an artificial substance which possesses the principle characteristic properties of tannin. Phil. Trans. 1805, p. 211, and 285.

The

The coal, amounting to 30.5 grains, yielded on incineration 3 grains of lime. To discover whether any fixed alkali was present, 200 grains of the purest guaiacum (that in drops) were reduced to ashes: these were dissolved in muriatic acid, and precipitated by ammonia: the whole was then filtrated, and the clear liquor evaporated to dryness, but not any trace of a neutral salt with a basis of fixed alkali was preceptible.

Enumeration of the difference between guaiacum and resins.

Sect. IV.—From the action of different solvents on guaiacum, it appears, that although this substance possesses many properties in common with resinous bodies, it nevertheless differs from them in the following particulars:

1. By affording a portion of vegetable extract.

2. By the curious alterations which it undergoes when subjected to the action of bodies, which readily communicate oxigen, such as nitric and oxy-muriatic acids: and the rapidity with which it dissolves in the former.

3. By being converted into a more perfect resin: in which respect guaiacum bears some resemblance to the green resin which constitutes the colouring matter of the leaves of trees, &c.*

4. By yielding oxalic acid.

5. By the quantity of charcoal and lime which are obtained from it when subjected to destructive distillation.

Guaiacum appears to be, or to contain, a peculiar substance.

Sect. V.—From the whole therefore of the above-mentioned properties, it evidently appears that guaiacum is a substance very different from those which are denominated resins, and that it is also different from all those which are enumerated amongst the balsams, gum resins, gums, and extracts: most probably it is a substance distinct in its nature from any of the above, in consequence of certain peculiarities in the proportions and chemical combination of its constituent elementary principles; but as this opinion may be thought not sufficiently supported by the facts which have been adduced, we may for the present be allowed to regard guaiacum as composed of a resin modified by the vegetable extractive principle,

* This substance was found by Proust to be insoluble in water, and soluble in alcohol. When treated with oxy-muriatic acid, it assumed the colour of a withered leaf, acquiring the resinous properties in greater perfection. Vide Thompson's Syst. of Chemistry, 2d edit. vol. iv. p. 318.

and as such, perhaps the definition of it by the term of an *extracto-resin* may be adopted without impropriety.

Oxigenation of that part of matter which alcohol takes up.

P. S.—I have observed that the action of oxigen on some of the other resinous bodies is very remarkable. It is well known that by digesting mastich in alcohol, a partial solution only is formed, and there remains an elastic substance, which is generally said to possess the properties of pure caoutchouc; it appears however to differ from this substance in becoming hard when dried by exposure to air. Moreover, I have remarked that the part of mastich which remains dissolved by alcohol, may be again precipitated by water. and, on examination, I found the precipitate to possess the properties of a pure resin: but when a stream of oxy-muriatic acid gas was made to pass through the solution, a tough elastic substance was thrown down, which became brittle when dried, and was soluble in boiling alcohol, but separated again as the solution cooled: its properties, therefore, somewhat approached to those of the original insoluble part.

VI.

On Silver Coins. By Thomas Thomson, *M.D. F.R.S. E. Communicated by the Author.*

Silver as a medium of exchange.

SILVER has been employed by most nations as a medium of exchange. The ancients appear to have coined the metal pure, and the same practice is still followed, I believe, in some of the Eastern nations. But in Europe it is always alloyed with copper; pure silver being considered as too soft for coin.

Analysis of silver coins.

The analysis of silver coins is not attended with much difficulty. The metals which they contain are silver and copper, and almost always a little gold. The method which I use to separate and estimate the relative weights of these metals was the following:

The method.

1. The silver coin was first well cleaned by means of soap or an alkaline ley. It was afterwards weighed, and its speci-

fic

fic gravity ascertained. The specific gravity of silver varies, as is well known, according to circumstances. I melted two ounces of pure silver, and let it cool in the bottom of a black-lead crucible previously heated. Its specific gravity was 10.3946. The same mass was fused a second time, and cast into a thin plate. Its specific gravity was now reduced to 10.1790. When this plate had been passed between rollers, its specific gravity was found to be 10.4812. By hammering the round button, its specific gravity became 10.4177.

Specific gravity of pure silver.

The coin after being cleaned was put into a matrass with a sufficient quantity of pure nitric acid previously diluted with about twice its bulk of water. The solution as soon as the acid ceased to act was poured off, and the black powder which usually remained, was repeatedly digested on a sand bath with small quantities of nitric acid. It was then washed with distilled water, and dissolved in nitromuriatic acid. The solution was mixed with liquid sulphate of iron, and the blackish powder which fell was washed, and formed into a solid mass, sometimes by amalgamating it with mercury, and driving off the volatile metal by heating the amalgam in a small porcelain crucible; sometimes by forming it into a ball with calcined borax and fusing it into a button before the blow-pipe. The metals thus obtained was the gold. It never exceeded $\frac{1}{200}$th part of the coin, and seldom amounted to $\frac{1}{1000}$th part. In some coins, no gold whatever could be detected. In some of the following analysis, the black powder which remained when the coin was first dissolved in nitric acid, was melted into a button, and weighed. This button was then treated with nitromuriatic acid: if it dissolved completely, it was considered as pure gold; but if it left any residue of muriate of silver, this muriate was carefully dried and weighed, and the proportion of silver thus indicated was subtracted from the weight of the button; the remainder was considered as the weight of the gold contained in the coin.

Solution of the coin in nitric acid diluted.

The undissolved gold dissolved in nitromuriatic acid and precipitated, &c.

3. The nitric acid solution was mixed with a solution of common salt, more than sufficient to separate the whole of the silver. The muriate of silver was allowed to settle at the bottom of the vessel, and the liquid carefully decanted off. Distilled water was poured upon the precipitate, the mixture was well stirred with a glass rod, and left at rest till the muriate was deposited; then the water was decanted off, and a new

The nitric solution precipitated by com. salt and the muriate of silver weighed.

new portion substituted. This was repeated till the water came off perfectly pure. The precipitate was then washed out of the vessel into a glass or porcelain capsule, and the water being drained, it was dried for two hours upon a sand bath, heated nearly to the temperature of 400° the vessel was then carefully weighed, first with the muriate in it, and then after that substance had been removed, the difference gave the exact weight of muriate of silver dried in a heat rather under 400°.

Reduction of the muriate.

In the eariler analyses, the silver was reduced from this muriate by fusing it with common potash. But this method was found liable to some uncertainty. If the heat be suddenly raised, a portion of the muriate of silver is apt to be volatilized, even though covered with potash; and even when we succeed in preventing this, it is but seldom that the whole of the silver is united in a single mass. Small globules very frequently sink into the crucible, and can with difficulty be collected. For these reasons, I found it more convenient, as well as precise, to estimate the weight of the silver from the dried muriate.

Standard experiment to reduce the silver contained in the muriate.

A hundred grains of pure silver, reduced from the muriate, were dissolved in nitric acid, and evaporated gently till the whole was brought to a state not unlike that of calcined borax. The weight was 157.18 grains. When the heat was continued, fumes of nitrous acid soon filled the retort, and the silver was reduced, appearing in the state of fine crystalline flakes. A hundred grains of pure silver were dissolved in nitric acid, evaporated to dryness, redissolved in water, and precipitated by muriate of soda; the precipitate, being well washed, was placed for two hours on a sand bath, heated nearly to 400°. It was allowed to cool, and then weighed. It was again placed on the sand bath for two hours more, but the weight was not altered. It was allowed to stand for two days, exposed to the open air, in a dry room, but no alteration in its weight took place: the weight was 132.35 gr. I then put the glass capsule, containing the muriate, into a crucible, surrounded it with sand, and brought the muriate to fusion: it now weighed only 128.67 gr. From this experiment it follows, that muriate of silver, dried at a heat of nearly 400°, contains 0.7554 of silver; and when melted, 0·7764. A repetition of the experiment gave very nearly the same result.

sult. Hence, to find the quantity of silver in muriate of silver dried at a heat of nearly 400°, we have only to multiply its weight by 0.756. This was the method which I followed. It corresponds very nearly with the result of former analyses as made by others.

4. The solution thus freed from the silver, and containing a considerable excess of muriate of soda, was mixed with all the water employed to wash the muriate of silver, and evaporated to dryness in a porcelain capsule. The dry mass was dissolved in water. Sometimes a little muriate of silver separated during the solution. It was always carefully washed, and added to the precipitate of silver previously obtained. Residue of the solution evaporated and redissolved.

A polished plate of iron was then put into the liquid, which was diluted with water, if necessary, till it just covered the upper end of the plate; it was then laid aside till the whole of the copper was thrown down. Two days were usually required for this separation; sometimes longer, sometimes a shorter time sufficed. Care was taken not to disturb the liquid during the process; for when the copper falls down, the separation is always more tedious. When the process was finished, the plate of iron was withdrawn, and the copper washed from it in distilled water. A portion of the copper often fell down when the plate was withdrawn. As soon as it had subsided, the liquor was decanted off, and water, acidulated with muriatic acid, poured upon the copper. This also was poured off after a few minutes, and pure water substituted in its place. The portion of copper washed off the iron plate was edulcorated in the same manner with water acidulated with muriatic acid. The whole was then collected on a filter, and carefully washed. It was allowed to dry in the open air, and afterwards placed for some hours on a steam bath. It was then weighed, and considered as the proportion of copper contained in the coin. Precipitation of the copper by iron.

As copper in the metallic state does not combine with water, the powder thus obtained is easily dried. Indeed, if it be thoroughly dried in the open air, it loses no sensible weight afterwards, though heated to 300°. It cannot be heated to redness, because even though this be done in a covered crucible, it very rapidly combines with oxygen, and is converted into a black powder. If it be heated to redness in an open crucible, 100 grains generally increase in weight to 120 grains. Desiccation of the copper.

Almost

Clean polished iron precipitates almost all the copper, &c.

Almost the whole of the copper may be precipitated from its solutions by means of a plate of iron, provided the iron be well polished and pure. If it be rough, a portion of the copper gets into the interstices, and cannot be separated. I dissolved 50 grains of pure granular copper (sp. gr. 8.6233) in sulphuric acid, evaporated to dryness, redissolved in water, and put into the solution a polished plate of iron. The copper precipitated weighed 49.5 gr., not reckoning a little which adhered to one side of the iron plate. Here the loss was only 1 per cent. 100 grains of copper dissolved in muriatic acid, were recovered with rather less loss; but when I employed a rough plate of iron, there was a loss of more than 6 per cent.; for 68 grains of copper, dissolved in muriatic acid, gave only 65.43 grains.

When copper is held in solution by nitric acid, it is thrown down partly in the metallic state, and partly oxidized. If muriatic acid be poured upon the precipitate, the whole is dissolved almost immediately, and converted into colourless muriate of copper. But if common salt be poured into the nitric acid solution, and the whole be evaporated to dryness, to dissipate any excess of acid, and redissolved in water, acidulated, if necessary, with muriatic acid, then iron throws down the copper in the metallic state.

I did not succeed so well in my attempts to precipitate copper by means of a cylinder of zinc. That metal usually became porous, and it was difficult to separate the copper from it. Besides, part of the copper was thrown down in the state of an alloy, for it effervesced when washed with muriatic acid.

Curious fact.

A curious phenomenon repeatedly occured when diluted muriatic acid was poured upon copper precipitated in this manner by zinc. The effervescence was sudden and strong, and the gas separated was *nitrous gas*, as became evident by the red fumes generated.

The specific gravity of the fine powder of copper separated from acid solutions by iron, I found only 7.056 in the temperature of 63°; but, by simply fusing it with black flax, the specific gravity became 8.535.

Coins which were analysed.

SECT. II.—The following Table exhibits a list of the different coins which I have analysed, placed in the order of analysis; together with the products obtained.

An

1. An English half-crown. It was a coin of Charles II. dated 1671, and was one of the beautiful pieces coined by Simeon. It weighed 220.5 grains, and had of course sustained a loss of 11 grains.

English half-crown: Silver 12, copper 1,

Muriate of silver, 267.51 gr. =201.23 silver	
Copper	17.00
Loss, including a trace of gold	2.27
	220.5

2. A French half-crown. It was a coin of Louis XV. dated 1761. It weighed 211.5 grains.

French half-crown: Silver 10, copper 1.

Silver obtained by reducing the muriate	190.5 grains
Copper	19.2
Gold	0.3
Loss	1.5
	211.5

3. A rupee. It was brought from India by Mr. Philip Dundas, and given me by his nephew, Mr. Colt. It weighed 178 grains. By an accident, the glass containing it was overturned and broken soon after the solution in nitric acid began. When washed and dried, the rupee still weighed 152.5 grains. It was this portion only that was analysed.

Rupee: Silver 32½, copper 1.

Silver reduced from the muriate	146.5 grains
Copper	4.5
Loss, including some gold	1.5
	152.5

4. A Spanish pisterine. It was a coin of Louis I. and dated 1724. It was brought from Spain by Mr. Farquharson, of Haughton, from whom I obtained it. It weighed 85.5 grains.

Spanish pisterine: Silver 5½, copper 1.

Muriate of silver 94 gr. =71 silver	
Copper	13
Loss, with a little gold	1.5
	85.5

Portugal half-crusade: Silver $8\frac{1}{2}$, copper 1.

5. A half-crusada nova of Portugal. This coin is usually called a twelve vintem piece. It was a coin of Mary I. and Peter III. dated 1782. It weighed 117 grains.

Silver reduced from the muriate	103 grains
Copper	12
Loss	2
	117

Sardinian coin: Silver $9\frac{1}{2}$, copper 1.

6. A Sardinian coin of Victor Amadæus, dated 1779. It weighed 135.5 grains.

Silver	122 grains
Copper	13
	135

Coin of Berne: Silver $3\frac{1}{2}$, copper 1.

7. A coin of the canton of Berne, dated 1717. The letter *B* surmounted with a crown. Inscription: *Dominus providebit.* *Reverse*, the arms of Berne. Inscription: *Moneta Reipublicæ Bernensis.—Cr.* 20. It weighed 73 grains.

Muriate of silver 76 gr.=57.5 silver	
Copper, by estimate	15.5
	73.0

For this and the four preceding coins, as well as for the following, I am indebted to Francis Farquharson, Esq, of Haughton, who brought them from the Continent.

Ancient coin, Greek: Silver nearly pure.

8. A coin of Crotona. This is one of the oldest of the Greek silver coins. It is supposed to have been coined about 600 years before the commencement of the Christian æra. It was rude, thick, not quite round; hollow on the *reverse;* with three legs, somewhat in the shape of an *m*, on the *obverse*. There was the appearance of letters, but too much effaced to be decyphered. The weight was 113.64 grains.

Muriate of silver, 144.96 gr.=	109.50 silver
Copper	1.00
Gold	.13
Loss	3.01
	113.64

9. A

9. A Dutch guilder, dated 1791, *Obverse*, the arms of the Republic, with the words, *Mo. Arg. Ord. Fœd. Belg. Holl. Reverse*, a female figure leaning on a pillow, and holding a spear. Inscription: *Hanc tuemur, hac nitimur.* It weighed 163.93 grains. It was brought from Holland by William Sligo, Esq., from whom I got it for analysis. Dutch guilder: Silver 15, copper 1.

Muriate of silver, 198.90 gr.=150.37 silver	
Copper	10.72
Gold	.06
Loss	2.78
	163.93

10. A Russian piece of 15 copecs, dated 1789. *Obverse*, the head of Catharine II. with the usual title. *Reverse*, the arms of Russia, with the figures 15. It weighed 52.97 grains. It was brought from Russia by Mr. Hatchett, from whom I got it for the purpose of analysis. Russian coin: Silver 4, copper 1.

Muriate of silver, 52.97 gr.=40.04 silver	
Copper	11.39
Loss	1.54
	52.97

In this coin there was not the smallest trace of gold to be detected.

11. A Scotch coin of Charles I. *Obverse*, the head of the king, with the number XL. (40 shillings Scotch). Inscription: *Car. D. G. Scot. Ang. Fr. et Hib. R. Reverse*, a thistle with a crown, and the motto, *Salus Reipublicæ Suprema Lex.* It weighed 27.05 grains. Its specific gravity was 10.000. Scotch 40 shilling: Old silver 12, copper 1.

Muriate of silver, 33.02 gr.=24.96 silver	
Copper	1.90
Loss	0.19
	27.05

This coin was found in digging the foundation of one of the houses in the new town of Edinburgh by a common mason, who brought it to me.

 12. A

Hambro. coin: Silver 11, copper 10.

12. A coin of Hamburg, dated 1780. *Obverse*, the value of the coin; 12 *einen Thal*. *Reverse*, a horse. This coin was likewise given me by Mr. Hatchett. It weighed 50.44. Its specific gravity was only 9.0154.

Muriate of silver, 33.53 gr.=25.35 silver.	
Copper	22.71
Loss	2.36
	50.44

The great quantity of copper in this coin led me at first to suspect that it might have been accidentally debased. This made me anxious to examine another of the same kind. Professor Jameson furnished me with one which he had brought from Germany. The result was as follows:

The same coin, rather worse.

13. A similar coin, rather smaller in diameter, but thicker, dated 1794. It weighed 46.7 grains.

Muriate of silver, 31.52 gr.=23.83 silver	
Copper	22.60
Loss	0.27
	46.7

Having been informed by Mr. Milligan, watch-case maker, in Edinburgh, that the Spanish coins differed from each other in purity, I examined the two following. The first was furnished by that gentleman as a specimen of the purest Spanish silver coin; the second was a common Spanish dollar.

Spanish pisterine: Silver 18, copper 1.

14. A Spanish pisterine of Philip V. dated 1740. It weighed 99.07 grains.

Muriate of silver, 120.30 gr.=90.95 silver	
Copper	5.53
Loss, including some gold	2.59
	99.07

15. A

15. A Spanish dollar, dated 1801. It weighed 415.16 gr. Its specific gravity was 10.291.

Spanish dollar, very new: Silver 9, copper 1.

Muriate of silver, 490.33 gr.=370.69 silver	
Copper	42.29
Gold	0.29
Loss	1.89
	415.16

16. A Danish 60 schilling piece, dated 1789. *Obverse*, the head of the king of Denmark, with the inscription, *Christianus VII. D. G. Dan. Norv. V. G. Rex*. *Reverse*, the arms of Denmark, with the words, 60 *Schelling. Schlesw. Holst. Courant.* It weighed 444.55 grains. Its specific gravity was 10.2667. I received it from Professor Jameson, who brought it from Germany.

Danish coin: Silver $7\frac{1}{2}$, copper 1.

Muriate of silver, 516.98 gr.=390.63 silver	
Copper	53.68
Gold	0.09
Loss	.15
	444.55

17. A Roman denarius at the time of the republic. *Obverse*, the head of a warrior. *Reverse*, Diana drawn in a car by two stags; below, a crescent, and the word *Roma*. It weighed 60.06 grains. Its specific gravity was 10.463. I received this, as well as the following coin, from the collection of Francis Farquharson, Esq. of Haughton.

Roman denarius: Silver nearly pure.

Muriate of silver, 78.94 gr.=59.68 silver	
Gold	0.29
Copper, by estimate	0.09
	60.06

The presence of copper was rendered manifest when the last solution was concentrated and mixed with ammonia. But I could not succeed in separating and weighing it.

18. A

Denarius of Domitian: Silver 4, copper 1.

18. A denarius of the emperor Domitian. On one side the head of Domitian. Inscription, *Domitianus Aug. P.M. Imp.* and four letters effaced. *Reverse*, a warrior with a spear and shield. The inscription too much effaced to be read. It weighed 52.28 grains. Its specific gravity was 10.092.

Muriate of silver, 55.35 gr. = 41.84 silver	
Copper	10.02
Gold	.30
Loss	.12
	52.26

Austrian crown: Silver 10, copper 1.

19. An Austrian crown, dated 1612. *Obverse*, the head of Matthias II. with the inscription, *Matthias II. D. G. Hung. Bohe. Rex.* *Reverse*, the arms of these countries quartered, with the words, *Arch. Aust. Dux. Burg. Marg. Mo. Got.* It weighed 445.96 grains. Its sp. gr. was 10.233. I received it from Professor Jameson.

Muriate of silver, 533.5 gr. = 403.3 silver	
Silver separated from the gold	.12
Copper	41.86
Gold	.40
Loss	.28
	445.96

SECT.

SECT. III.

The following TABLE exhibits the composition of 100 parts of each of the Coins examined, according to the preceding analysis.

Names of the Coins.	Silver	Copper.	Gold.	Loss.	Total.
1. ANCIENT.					
Grecian Coin of Crotona . . .	96.27	0.88	0.11	2.74	100
Roman denarius of the republic	99.37	0.15	0.48	—	100
——— denarius of Domitian	80.03	19.17	0.45	0.35	100
2. MODERN.					
Rupee	96.06	2.95	—	0.99	100
British. English half-crown .	91.26	7.71	—	1.03	100
———Scotch 40 shilling piece	92.41	7.03	—	0.36	100
French half-crown	90.07	9.08	0.14	0.71	100
Spanish pisterine	83.04	15.20	—	1.76	100
——— pisterine	91.80	5.58	—	2.62	100
——— dollar	89.28	10.18	0.07	0.47	100
Portuguese. A 12 vintem piece	88.03	10.25	—	1.72	100
Dutch guilder	91.72	6.54	0.04	1.70	100
Sardinian	90.04	9.96	—	—	100
Swiss	78.77	21.22	—	—	100
German. Hamburgh	51.03	48.39	—	0.58	100
——— Ditto	50.25	45.02	—	4.73	100
——— Austrian crown . .	90.47	9.38	0.09	0.06	100
Danish. A 60 schelling piece	87.87	12.07	0.02	0.04	100
Russian. A 15 copec piece . .	75.59	21.50	—	2.91	100

The

Gold accidental in silver coins.

The small quantity of gold which was found in these coins must be considered as altogether accidental, and ought to be added to the silver when we estimate the proportion of alloy. The quantity of gold found in the ancient coins, especially the Roman, considerably exceeds that found in modern coins. In the denarius of the republic it amounted to nearly $\frac{1}{250}$ th part of the whole.

Ancient coins pure.

The ancient Greek and Roman coins appear to have been formed of pure silver. The small quantity of alloy which they contain was doubtless accidental. So minute a proportion could not have been added on purpose. But the denarius of Domitian contains 19.5 of copper. During the reign of this emperor then, it seems, the Romans were in the practice of alloying their gold with copper.

Rupee almost pure.

The rupee likewise appears to be almost pure silver. The alloy, which amounts only to $\frac{1}{33}$d, is most probably rather accidental than added on purpose. But there are various kinds of rupees current in India, and perhaps they may differ in their purity.

European silver coins all alloyed, &c.

All the European silver coins are alloyed with copper, but the proportion varies considerably. The British is the purest examined, and the Hamburgh the least pure. The following Table exhibits the proportion of alloy in these coins, arranged according to their purity. I have added the loss sometimes to the copper, sometimes to the silver, and sometimes to both, according as I suspected from the method of conducting the analysis, that the deficiency was occasioned by the loss of one or other metal, or of both.

Relative purity of coins.

	Alloy per cent.	Weight of Silver, that of the Copper being 1.
British	7.5	12.5
Dutch	8	11.5
French	9	10.1
Austrian	9.5	9.5
Sardinian	9.5	9.5
Spanish	10.5	8.5
	15.5	5.5
Portuguese	11	8
Danish	12	7.3
Swiss	21	3.8
Russian	24	3.6
Hamburgh	50	1

The first column of this Table gives the supposed proportion of alloy in 100 parts of the respective coin; the second gives the weight of silver contained in each coin, on the supposition that the weight of the copper with which the silver is alloyed is always 1.

VII.

On the Direction of the Radicle and Germen during the Vegetation of Seeds. By THOMAS ANDREW KNIGHT, *Esq. F.R.S. In a Letter to the Right Hon. Sir Joseph Banks, P.R.S.**

MY DEAR SIR,

IT can scarcely have escaped the notice of the most inattentive observer of vegetation, that in whatever position a seed is placed to germinate, its radicle invariably makes an effort to descend towards the centre of the earth, whilst the elongated germen takes a precisely opposite direction: and it has been proved by Du Hamel,* that if a seed during its germination, be frequently inverted, the points both of the radicle and germen will return to the first direction. Some naturalists have supposed these opposite effects to be produced by gravitation: and it is not difficult to conceive that the same agent, by operating on bodies so differently organized as the radicle and germen of plants are, may occasion the one to descend and the other to ascend.

Vertical position in which vegetables grow,

—ascribed to gravitation:

The hypothesis of these naturalists does not, however, appear to have been much strengthened by any facts they were able to adduce in support of it, nor much weakened by the arguments of their opponents; and, therefore, as the phenomena observable during the conversion of a seed into a plant are amongst the most interesting that occur in vegetation, I commenced the experiments, an account of which I have now the honour to request you to lay before the Royal Society.

Not yet proved by facts.

* Philos. Trans. 1806. † Physique des Arbres.

Probability that trial might be made of this by constant change of the position of the seed.

I conceived, that if gravitation were the cause of the descent of the radicle, and of the ascent of the germen, it must act either by its immediate influence on the vegetable fibres and vessels during their formation, or on the motion and consequent distribution of the true sap afforded by the cotyledons; and as gravitation would produce these effects only whilst the seed remained at rest, and in the same position relative to the attraction of the earth, I imagined that its operation would become suspended by constant and rapid change of the position of the germinating seed, and that it might be counteracted by the agency of centrifugal force.

Beans were fastened in all positions to the circumference of an upright revolving wheel.

Having a strong rill of water passing through my garden, I constructed a small wheel, similar to those used for grinding corn, adapting another wheel of a different construction, and formed of very slender pieces of wood, to the same axis. Round the circumference of the latter, which was eleven inches in diameter, numerous seeds of the garden bean, which had been soaked in water to produce their greatest degree of expansion were bound, at short distances from each other. The radicles of these seeds were made to point in every direction; others as tangents to its curve; some pointing backwards, and others forwards, relative to its motion; and others pointing in opposite directions in lines parallel with the axis of the wheels. The whole was inclosed in a box, and secured by a lock, and a wire grate was placed to prevent the ingress of any body capable of impeding the motion of the wheels.

—which performed 150 revolutions in a minute.

The water being then admitted, the wheels performed something more than 150 revolutions in a minute; and the position of the seeds relative to the earth was of course as often perfectly inverted within the same period of time; by which I conceive, that the influence of gravitation must have been wholly suspended.

The seeds grew with the germens directed to the centre, and the radicles in the opposite direction.

In a few days the seeds began to germinate; and as the truth of some of the opinions I had communicated to you, and of many others which I had long entertained, depended on the result of the experiment, I watched its progress with some anxiety, though not with much apprehension; and I had soon the pleasure to see that the radicles, in whatever direction they were protruded from the position of the seed, turned their points outwards from the circumference of the wheel, and in their subsequent

subsequent growth receded nearly at right angles from its axis. The germens, on the contrary, took the opposite direction, and in a few days their points all met in the centre of the wheel. Three of these plants were suffered to remain on the wheel, and were secured to its spokes to prevent their being shaken off by its motion. The stems of these plants soon extended beyond the centre of the wheel: but the same cause which first occasioned them to approach its axis, still operating, their points returned and met again at its centre.

Repetition of the experiment with an horizontal wheel.

The motion of the wheel being in this experiment vertical, the radicle and germen of every seed occupied, during a minute portion of time in each revolution, precisely the same position they would have assumed had the seeds vegetated at rest; and as gravitation and centrifugal force also acted in lines parallel with the vertical motion and surface of the wheel, I conceived that some slight objections might be urged against the conclusions I felt inclined to draw. I therefore added to the machinery I have described, another wheel, which moved horizontally over the vertical wheels; and to this, by means of multiplying wheels of different powers, I was enabled to give many degrees of velocity. Round the circumference of the horizontal wheel, whose diameter was also eleven inches, seeds of the bean were bound as in the experiment which I have already described; and it was then made to perform 250 revolutions in a minute. By the rapid motion of the water-wheel, much water was thrown upwards on the horizontal wheel, part of which supplied the seeds upon it with moisture, and the remainder was dispersed, in a light and constant shower, over the seeds in the vertical wheel, and on others placed to vegetate at rest in different parts of the box.

The radicles grew obliquely outwards and downwards, and the germens obliquely inwards and upwards, and more so the swifter the motion.

Every seed on the horizontal wheel, though moving with great rapidity, necessarily retained the same position relative to the attraction of the earth; and therefore the operation of gravitation could not be suspended, thought it might be counteracted, in a very considerable degree, by centrifugal force: and the difference, I had anticipated, between the effects of rapid vertical and horizontal motion, soon became sufficiently obvious: the radicles pointed downwards about 10 degrees below, and the germens as many degrees above, the horizontal line of the wheels' motion; centrifugal force having made both

 to

to deviate 80 degrees from the perpendicular direction each would have taken, had it vegetated at rest. Gradually diminishing the rapidity of the motion of the horizontal wheel, the radicle descended more perpendicularly, and the germens grew more upright; and when it did not perform more than 80 revolutions in a minute, the radicle pointed about 45 degrees below, and the germen as much above, the horizontal line, the one always receding from, and the other approaching to, the axis of the wheel.

Remarks on the degree of accuracy of the experiments.

I would not, however, be understood to assert that the velocity of 250 or of 80 horizontal revolutions in a minute will always give accurately the degrees of depression and elevation of the radicle and germen which I have mentioned; for the rapidity of the motion of my wheels was sometimes diminished by the collection of fibres of conferva against the wire grate, which obstructed, in some degree, the passage of the water: and the machinery, having been the workmanship of myself and my gardener, cannot be supposed to have moved with all the regularity it might have done, had it been made by a professional mechanic. But I conceive myself to have fully proved that the radicles of germinating seeds are made to descend, and their germens to ascend, by some external cause, and not by any power inherent in vegetable life; and I see little reason to doubt that gravitation is the principal, if not the only agent employed in this case by nature. I shall therefore endeavour to point out the means by which I conceive the same agent may produce effects so diametrically opposite to each other.

Observations on the means by which gravity produces the vertical position in the radicle.

The radicle of a germinating seed (as many naturalists have observed) is increased in length only by new parts successively added to its apex or point, and not at all by any general extension of parts already formed: and the new matter which is thus successively added, unquestionably descends in a fluid state from the cotyledons.* On this fluid, and on the vegetable fibres and vessels whilst soft and flexible, and whilst the matter which composes them is changing from a fluid to a solid state, gravitation, I conceive, would operate sufficiently to give an inclination downwards to the point of the radicle; and as the radicle has been proved to be obedient to centrifugal force, it can scarcely be contended that its direction would remain uninfluenced by gravitation.

* See Philos. Trans. 1805.

I have

I have stated that the radicle is increased in length only by parts successively added to its point. The germen, on the contrary, elongates by a general extension of its parts previously organized; and its vessels and fibres appear to extend themselves in proportion to the quantity of nutriment they receive. If the motion and consequent distribution of the true sap be influenced by gravitation, it follows, that when the germen at its first emission, or subsequently, deviates from a perpendicular direction, the sap must accumulate on its upper side; and thence it follows, that the point of the germen must always turn upwards. And it has been proved, that a similar increase of growth takes place on the external side of the germen when the sap is impelled there by centrifugal force, as it is attracted by gravitation to its under side when the seed germinates at rest.

And also in the germen.

This increased elongation of the fibres and vessels of the under side is not confined to the germens, nor even to the annual shoots of trees, but occurs and produces the most extensive effects in the subsequent growth of their trunks and branches. The immediate effect of gravitation is certainly to occasion the further depression of every branch, which extends horizontally from the trunk of the tree; and when a young tree inclines to either side, and thus occasions an increased longitudinal extension of the substance of the new wood on that side,* the depression of the lateral branch is thus prevented; and it is even enabled to raise itself above its natural level when the branches above it are removed; and the young tree, by the same means, becomes more upright, in direct opposition to the immediate action of gravitation, nature, as usual, executing the most important operations by the most simple means.

The same effect of gravitation takes place in the branches of trees.

I could adduce many more facts in support of the preceeding deductions; but those I have stated, I conceive to be sufficiently conclusive. It has, however, been objected by Du Hamel (and the greatest deference is always due to his opinions), that gravitation could have little influence on the direction of the germen, were it in the first instance protruded, or

Inversion of the seed after protrusion of the germen does not prevent the effect.

* This effect does not appear to be produced in what are called weeping trees; the cause of which I have endeavoured to point out in a former memoir. Philos. Trans. 1804.

were

were it subsequently inverted, and made to point perpendicularly downwards. To enable myself to answer this objection, I made many experiments on seeds of the horse-chesnut, and of the bean, in the box I have already described: and as the seeds there were suspended out of the earth, I could regularly watch the progress of every effort made by the radicle and germen to change their positions. The extremity of the radicle of the bean, when made to point perpendicularly upwards, generally formed a considerable curvature within three or four hours, when the weather was warm. The germen was more sluggish; but it rarely or never failed to change its direction in the course of twenty-four hours; and all my efforts to make it grow downwards, by slightly changing its direction, were invariably abortive.

Another, and apparently a more weighty, objection to the preceding hypothesis (if applied to the subsequent growth and forms of trees), arises from the facts that few of their branches rise perpendicularly upwards, and that their roots always spread horizontally; but this objection, I think, may be readily answered.

Causes why the branches of trees do not exactly obey this operation.

The luxuriant shoots of trees, which abound in sap, in whatever direction they are first protruded, almost uniformly turn upwards, and endeavour to acquire a perpendicular direction; and to this their points will immediately return if they are bent downwards during any period of their growth; their curvature upwards being occasioned by an increased extension of the fibres and vessels of their under sides, as in the elongated germens of seeds. The more feeble and slender shoots of the same trees will, on the contrary, grow in almost every direction, probably because their fibres, being more dry, and their vessels less amply supplied with sap, they are less affected by gravitation. Their points, however, generally shew an inclination to turn upwards; but the operation of light, in this case, has been proved by Bonnet * to be very considerable.

Why the roots of trees extend horizontally.

The radicle tapers rapidly as it descends into the earth, and its lower part is much compressed by the greater solidity of the mould into which it penetrates. The true sap also

* Recherches sur l'Usage des Feuilles dans les Plantes.

continues

continues to descend from the cotyledons and leaves, and occasions a continued increase of the growth of the upper parts of the radicle; and this growth is subsequently augmented by the effects of motion when the germen has risen above the ground. The true sap, therefore, necessarily obstructed in its descent, numerous lateral roots are generated, into which a portion of the descending sap enters. The substance of these roots, like that of the slender horizontal branches, is much less succulent than that of the radicle first emitted, and they are in consequence less obedient to gravitation; and therefore meeting less resistance from the superficial soil, than from that beneath it, they extend horizontally in every direction, growing with most rapidity, and producing the greatest number of ramifications, wherever they find most wamth and a soil best adapted to nourish the tree. As these horizontal or lateral roots surround the base of the tree on every side, the true sap descending down its bark, enters almost exclusively into them; and the first perpendicular root having executed its office of securing moisture to the plant whilst young, is thus deprived of proper nutriment, and ceasing almost wholly to grow, becomes of no importance to the tree. The tap root of the oak, about which so much has been written, will possibly be adduced as an exception, but having attentively examined at least 20,000 trees of this species, many of which had grown in some of the deepest and most favourable soils of England, and never having found a single tree possessing a tap root, I must be allowed to doubt that one ever existed.

The notion of a tap root is unfounded.

As trees possess the power to turn the upper surfaces of their leaves and the points of their shoots to the light, and their tendrils in any direction to attach themselves to contiguous objects, it may be suspected that their lateral roots are by some means directed to any soil in their vicinity which is best calculated to nourish the plant to which they belong; and it is well known, that much the greater part of the roots of an aquatic plant which has grown in a dry soil, on the margin of a lake or river, have been found to point to the water; whilst those of another species of tree which thrives best in a dry soil, have been ascertained to take an opposite direction. But the result of some experiments I have made is not favourable to this hypothesis; and I am rather inclined to believe that the roots

Trees have a power of shooting towards moist or dry places according to their necessities.

disperse

disperse themselves in every direction, and only become more numerous where they find most employment and a soil best adapted to the species of plant. My experiments have not, however, been sufficiently varied, or numerous, to decide this question, which I propose to make the subject of future investigation.

I am, &c.
T. A. KNIGHT.

Elton, Nov. 22, 1805.

VIII.

On Thunder Storms. By Mr. SAMUEL BEREY.

To Mr. NCHOLSON.

SIR,

Edge Lane, near Liverpool, August, 1806.

THE many thunder storms we have had this summer, have brought to my mind a few remarks I was about to communicate to you, nearly nine years ago, but something particular at that time prevented me. As I have not seen them published by any one else, I now trouble you with them. Should you think them worthy of a place in your esteemed Journal, you will oblige me by inserting them, with any corrections you may be kind enough to make.

I am, Sir,
Your most obedient servant,
SAMUEL BEREY.

Franklin's discovery that lightning and electricity are the same.

Though the effects of thunder and lightning have been observed by all ages, yet the cause of these phenomena was a matter of conjecture, until Dr. Franklin's valuable experiments were made on lightning; by which he proved beyond a doubt, that lightning, and the electrical fluid, are precisely the same, as he was enabled to charge jars, and to perform experiments by the electrical fluid drawn from the clouds, with as great facility as by means of the electrical machine.

Having proved the similarity of lightning, and the electrical

cal fluid, the doctor naturally concludes that thunder is the sound caused by the discharge of a cloud containing an immense quantity of electrical fluid accumulated together, and says, "if two gun-barrels electrified, will strike at two inches "asunder, and make a loud report, what would be the effect "of perhaps ten thousand acres of electrified cloud."

If the discharge of an electrified cloud were the sole cause of thunder, and the clouds were charged to so large an extent, it is probable that the effects would be much more tremendous, and that fatal accidents would more frequently occur than they do at present; but it appears almost impossible that so large a quantity of the electrical fluid could be accumulated, particularly after the first discharge, when the equilibrium is restored, and the clouds, being full of wet vapour, are consequently become good conductors.

Remark, that thunder is not merely from electricity;

It seems more probable, that thunder is caused by the explosion of hydrogenous and oxigenous gas fired by the electrical fluid. It is well known that both these gases are constantly produced on the surface of the earth; that stagnated ponds, swamps, &c. emit carbonated hydrogen gas; and that plants are continually decomposing the air of the atmosphere, by imbibing the nitrogen, and emitting the oxigenous gas. This decomposition, and the formation of these gases, goes on more rapidly in warm than in cold weather.

—but from the explosion of hydrogen and oxigen.

On the commencement of a thunder storm, it is observable that the clouds are not very dark, but that after one or two explosions they become very black, owing to the gases being converted into water: the rain then becomes heavy, and small detached clouds fly towards the storm in every direction, not, I imagine, by the attraction of the electrical fluid, but to restore the equilibrium, and to fill up the vacuum caused by the gases having exploded and formed water, and consequently occupying a much less space than they did when in the state of gas. These clouds, by containing the mixed gases, may serve to feed the storm, or the moisture in them may become decomposed by the electrical fluid,* and

Observations on thunder storms.

* "Common fire, as well as electrical fire, gives repulsion to "the particles of water, and destroys their attraction of cohesion; "hence common fire, as well as electrical fire, assists in raising "vapours."—Franklin's Works, vol. i. page 206.

may

Observations on thunder storms, &c.

may form volumes of hydrogen and oxigen, which explode upon the next discharge of an electrified cloud.

If a thunder storm at a few miles distance be observed when the rest of the sky is tolerably clear, a quantity of white clouds, the bottom of them flat, but arched on the top, (owing, probably, to the resistance of the atmosphere against their rising, supposing them to contain the mixed gases, which are lighter than the common air of the atmosphere), will be seen floating above, and at the sides of the mass in which is the storm; by observing these white clouds attentively, some of them will be seen suddenly to become dark, and soon after to unite in the general mass. In this case, it appears that a partial explosion of the gases contained in those clouds must have taken place.

If thunder were caused merely by the discharge of an electrified cloud, and not by the gases exploding, the heaviest clap would always attend the largest and most vivid flash of lightning; but that is not the case, for a small clap of thunder frequently follows a large flash of lightning.

When the lightning takes an horizontal direction, there is frequently either no thunder, or a very small clap; but when it takes a perpendicular direction, the thunder is the heaviest. This is probably owing to the gases, which are formed during the storm, being collected above in considerable quantities. When I mention the lightning taking these different directions, I do not mean the electrical fluid, which appears blue, and frequently in a zig zag form, but that reddish light which surrounds it, and which has the appearance of a shadow, and may be frequently observed to extend to a great distance from the blue light, and which I conceive to be the gases in the act of exploding.

From the number of clouds that join the storm, the gases must frequently be in detached portions; and, when several of them are fired nearly at the same time, the explosions will be heard in several distinct claps, which may be observed in almost every thunder storm.

In hot weather, when it is rather windy, lightning is frequently observed unaccompanied by thunder; in this case, it appears that the wind has either blown the mixed gases abroad, or prevented their union.

From

From the above observations it appears probable, that the sound of thunder, and the formation of rain during a thunder storm, are caused by the explosion of volumes of hydrogenous and oxigenous gases fired by the lightning; and I am not aware of any phenomenon attending a thunder storm, but what may be easily accounted for upon the same theory. Thunder storms, &c.

IX.

Farther Communication from E. D. *respecting the Discoveries of Mayow.*

To Mr. NICHOLSON.

SIR,

I FORWARDED to you, a few days ago, some additional remarks on Lavoisier's claims, and stated some opinions of Mayow's concerning *acidification*. On looking again into that author, I have met with such further facts as I thought might be welcome to you, and request, Sir, that you will make such use of them as you think fit.

I am, Sir,

Your obedient servant,

E. D.

Speaking of the generation of nitrous acid, Mayow says: "Ut autem intelligatur, quo ritu spiritus acidus nitri in terra "generatur, liceat non nulla de *spiritu sulphuris*, cæterisque "liquoribus acidis præmittere: quippe spiritibus acidis qui- "buscunque similitudo maxima, et affinitas intercedit."— *Tractat. quinq.* p. 32. Mayow's induction, that sulphuric acid is formed by oxidation like the nitric.

He then combats the notion of the acidity of vitriol arising from an acid salt contained in the sulphur, and thinks the acid spirit may be formed during the deflagration of sulphur, from the mutual action of the sulphureous particles of the deflagrating matter, and the nitro-aerial particles of the air; which action he considered, however, to be of a mechanical nature. *Ibid.* p. 34. In support of these opinions, he refers to some circumstances occurring in the distillation of oil of vitriol, where he has these words: "Quippe experientiâ constat, quōd, si "distillatio vitrioli per decem, aut etiam plures dies cum igne That the acid i not in the sulphur.

"maximè intenso continuetur, spiritus tamen acidus usque in "receptaculum prodibit: verum enimvero vix credendum "est, spiritum quemvis acidum adeo fixum, et ponderosum "esse, qui tam diu in igne quàm violentissimo permanere pos-"sit: sed potius putandum est, particulas ignis nitro-aereas in "longâ illâ distillatione vitrioli cum sulphure congredi et ef-"fervescere."—*Ibid.* p. 36.

That vegetable empyreumatic acids are formed during distillation.

His opinions concerning the formation of the vegetable acids are thus expressed: "Præterea nescio, an non spiritus acidi e "lignis ponderosis, veluti ligno guaiaco, idque genus aliis dis-"tillati, simili ratione per ignis operationem inter distillan-"dum fiant: quippe lignum guaiaci ante distillationem neuti-"quam sale acido, sed potius sale fixo donari videtur. Huc "etiam facit, quòd particulæ nitro-aereæ ignis cum particulis "ligni sulphureis inter distillandum congressæ ad fluorem pu-"ducant. Illud etiam obiter annotamus, quòd spiritus acidi "e saccharo, et melle distillati, haud multum absimili ritū "per actionem spiritus nitro-aerei ignis fieri videantur."—*Ibid.* p. 38.

Spontaneous formation of sulphuric acid.

As to the spontaneous formation of vitriolic acid, he says, that if vitriol, from which the whole acid spirit has been expelled by calcination, be exposed again for some time to a moist air, it will be impregnated anew with the acid spirit.—*Ibid.* p. 38. Vitriols, he adds, are formed out of a saline sulphureous earth, which, when committed to the fire, will yield flowers of sulphur, and when exposed a due time to air and moisture, spontaneously ferments, and becomes richly impregnated with vitriol. "Nimirum spiritus nitro-aereus cum sul-"phure metallico marchasitarum istarum effervescens, partem "earum fixiorem in liquorem acidum convertit."—*Ibid.* p. 39. He further considers rust of iron to be produced by the action of these same nitro-aerial particles, which, combining with the metallic sulphur of the iron, form an acid that corrodes the metallic particles.—*Ibid.* p. 40. And, alluding to all these opinions in another part of his tract, he has these words: "Quanquam enim spiritus nitro-aereus "acidus non sit, ab eodem tamen ferrum corroditur, vitri-"ola conficiuntur, salia fixa ad fluorem perducuntur, re-"rumque compages tanquam ab universali menstruo solvun-"tur."—*Tractat. quinq.* p. 55.

Such,

Such, Sir, are the facts noticed by Mayow, which appeared to me so striking, that I have been induced to send them to you, to make hereafter what use of them you please. I have avoided, as much as possible, introducing the absurd theories by which he endeavoured to account for the facts he observed, and which do not in the least affect the truth of the observations themselves; and I have sent them mostly in his own language, that you might give them in that, or in an English dress, as you think fit, as well as for the purpose of presenting them to you in the most authentic form.

E. D.

X.

SCIENTIFIC NEWS.

New Map of Scotland.

MR. Arrowsmith has been for more than a year engaged in constructing a new map of Scotland, from original materials, to which he has obtained access by means of the Parliamentary Commissioners for making roads and building bridges in the Highlands of Scotland. New Map of Scotland.

The elaborate military survey of the main land of Scotland made in the middle of the last century, and preserved in his majesty's library, has been copied, and reduced for the present map; and the several proprietors of the Western Islands have communicated all their surveys, most of which have been very recently executed.

In addition to the astronomical observations heretofore known, many latitudes and longitudes have been purposely ascertained for this map, as well as a considerable number of magnetic variations.

This map is to be accompanied by a memoir, explanatory of the several documents on which it has been constructed.

The publication may be expected to take place in January or February next.

Medical

Medical Theatre, St. Bartholomew's Hospital.

Medical Theatre.

The following courses of lectures will be delivered at this theatre during the ensuing winter:

On the theory and practice of medicine, by Dr. Roberts and Dr. Powell.

On anatomy and physiology, by Mr. Abernethy.

On the theory and practice of surgery, by Mr. Abernethy.

On comparative anatomy and the laws of organic existence, by Mr. Macartney.

On chemistry, by Dr. Edwards.

On midwifery, and the diseases of women and children, by Dr. Thynne.

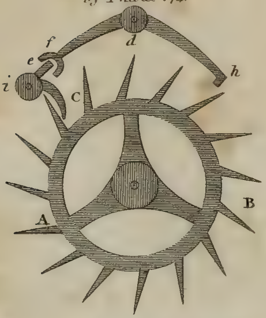
The anatomical demonstrations and practical anatomy, by Mr. Lawrence.

The anatomical lectures will begin on Wednesday, October 1, at 2 o'clock, and the other lectures in the course of the same week.

Further particulars may be known by applying to Mr. Nicholson, at the Apothecary's shop, St. Bartholomew's Hospital.

Detatched Escapement
by Thiout 1741

Fig 1



Count Rumfords Experiment on Heat
Fig 2

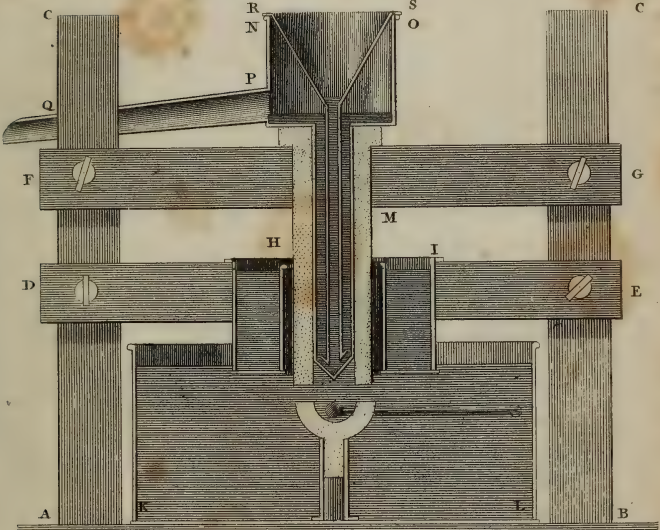


Fig 1

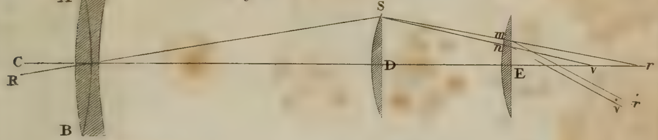


Fig 3

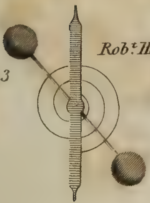
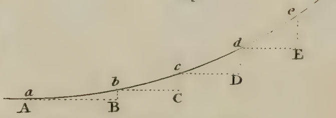


Fig 2



Mr. Hardy's Improvement on Pendulum Springs.

Fig 4

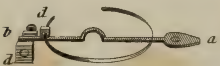


Fig 5

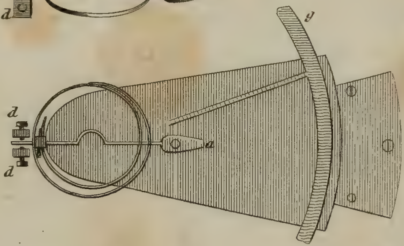
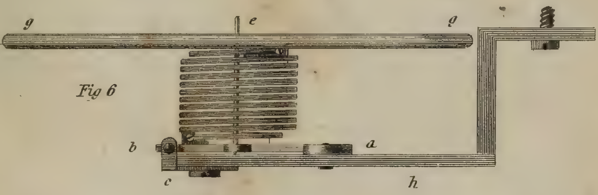


Fig 6



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